Assessing Universal Coverage:
Mexico's Seguro Popular

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Abstract

In the second half of the 20^{th} century, a series of reforms in Mexico's health system with the goal of expanding coverage gave rise to *Seguro Popular de Salud*, or Popular Insurance, a financial protection mechanism designed to guarantee the right for healthcare for every citizen. The intention of *Seguro Popular* was to expand access to health services for over 50 million uninsured individuals by eliminating out-of-pocket health payments with the ultimate aim to improve the population's health.

The objective of this thesis is to assess the impact of *Seguro Popular* on health outcomes, healthcare access, inequality and financial protection. The first chapter introduces the study and describes the programme. The second chapter is an assessment of the impact of *Seguro Popular* on Mexican population's health status and on utilisation of healthcare. Exploiting the time and space variation in the introduction of *Seguro Popular*, treatment effects are estimated using difference-in-differences specifications with variations in identification strategies. Results show that the programme had significant effects on reducing infant mortality rates. However, there were no clear effects on the adult health outcomes examined and on healthcare utilisation.

The aim of the third chapter is to find the effect of *Seguro Popular* on children's nutritional status; and to estimate the change in pure health inequality derived from the programme. Taking advantage of the staggered roll-out of *Seguro Popular*, children's height-for-age z-scores are assessed using a changes-in-changes approach. Results show that of *Seguro Popular* had a slightly positive effect on children's height. Moreover, the benefits were marginally larger for children in the lowest tail of the height distribution which represents a reduction of pure inequality in health.

The fourth chapter is an assessment of the antipoverty effect of *Seguro Popular*. Using instrumental variables, this study presents local average treatment effects of *Seguro Popular* on out-of-pocket healthcare expenditures, catastrophic expenditures and impoverishing. Results show a small reduction in out-of-pocket healthcare payments, although no clear effect on catastrophic payments or on impoverishing. However, the programme slightly reduced poverty overall and had a positive effect on households' welfare.

This thesis is dedicated to my parents

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Declaration

I declare that this thesis is a presentation of original work and I am the sole author. This work has not previously been presented for an award at this, or any other, university. All sources are acknowledged as references.

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Chapter 1

Preface

The three chapters on this thesis contribute to the existing literature that evaluates one of the most ambitious health reforms in Mexico in recent years: *Seguro Popular de Salud* (Popular Health Insurance, henceforth *Seguro Popular* or SP). The population of Mexico in 2000 was nearly 100 million; however, only half of its citizens had healthcare insurance at that time. *Seguro Popular* aimed to remove barriers to accessing healthcare by eliminating co-payments required of citizens when receiving health services. The end goal was to provide coverage to the approximately 50 million citizens who had no healthcare coverage. After being enacted into law, the programme was officially implemented in 2004, and progressively expanded throughout the country until national adoption was completed in 2012. *Seguro Popular* continued only until 2019, however, when the newly elected government cancelled the programme in favour of a new system.

1.1 The Mexican healthcare system

The public healthcare system in Mexico is composed of two main subsystems that provide most of the nation's health care: a social security system that serves formal sector employees and their families; and, a non-contributory system that covers the remainder of population, including people outside the formal sector of the economy, those who are self-employed, and the unemployed. According to the National System of Statistical and Geographical Information (INEGI),[1] 56.9% of Mexicans in 2019 were informally employed; this figure is likely even higher now, since, by the end of 2020, more than one million private businesses had to close as a consequence of the

[1]https://www.inegi.org.mx/contenidos/saladeprensa/boletines/2019/enoe_ie/enoe_ie2019_05.pdf

COVID-19 pandemic[2]. Notably, the majority of the population receives health care insurance via the non-contributory system.

There are large differences between these two sub-systems: formal sector employees and their families, either private sector workers or government employees, are entitled to receive health care services from the main public contributory sources of insurance, that is, social security. Social security benefits, including pensions, health care, daycare, disability and life insurance, among others, are provided by different institutions. The largest social security provider is the Mexican Social Security Institute (IMSS) which is financed mostly by payroll taxes and covers all formal private sector employees. Government employees are beneficiaries of a different social security system: the Government Worker's Social Security and Services Institute (ISSSTE), with equivalent benefits to IMSS. There are also other sources of insurance that account for a small fraction of the population including PEMEX (Mexican Petroleum) that provides social security for employees of state-run oil enterprise and SEDENA (National Defence).

The population in the informal sector are households not eligible for social security and therefore rely on government funded assistance services from the Ministry of Health (SSA) financed by the government through general revenues. Before the creation of *Seguro Popular*, this was a non-contributory system which charged a fee according to socioeconomic status. However, prior to 2002, per patient expenditure for individuals enrolled in SSA was about half the amount than for those enrolled in IMSS (Nigenda et al., 2015). Both systems are provided and subsidised by the government, but are independent systems with different governing bodies and completely independent infrastructures; and, by law, citizens could only be affiliated with either IMSS or SP.

The private sector is also divided into two main types of service providers with large variations in costs, quality and availability. Individuals with private insurance coverage or the capacity to pay for their health care out-of-pocket have access to a high quality modern network of private hospitals located in urban areas. Poorer urban and rural families without insurance use lower-priced health services with variable quality, including informal midwives and traditional healers (Flamand and Jaimes, 2015; Sosa-Rubi et al., 2009).

Table 1.1 compares the largest health providers in the public sector before the creation of *Seguro Poular*. Depending on the source, the proportion of the population who were covered by IMSS lies between 31 and 46 million[3]; the proportion

[2]INEGI. Estudio sobre la demografía de los negocios EDN 2020. https://www.inegi.org.mx/contenidos/programas/edn/2020/doc/EDN2020Pres.pdf

[3]The lower value is estimated by the national Survey of Health and Nutrition (ENSANUT 2000) and the higher value is reported by IMSS and ISSSTE as referenced in Table 1.1.

who were covered by ISSSTE was estimated to be between 5 and 10 million. Considering the nation's total population size was 97 million, the proportion of the population with no health insurance was estimated between 41 and 57 million – that is, approximately half. There was a wide difference in public spending per capita between the two systems; hence the need for a system to address this inequality.

Table 1.1 Healthcare coverage before *Seguro Popular* (2000)

Insurance	Covered population (millions)[a,b]	Public spending per capita (MXN)[c]
IMSS	31.51 - 46.53	4,654 - 3,151
ISSSTE	5.75 - 10.07	3,956 - 6,922
Not covered	57.5 - 40.98	680 - 954

a Encuesta Nacional de Salud y Nutrición (ENSANUT 2000)
b Centro de Estudios Sociales y de Opinión Pública, "Seguimiento y resultados de las políticas públicas y gestión gubernamental de la administración vigente: www.diputados.gob.mx/cesop/
c Per capita figures were calculated using budget information in Evolución del Gasto en Salud. Centro de Estudios de las Finanzas Públicas. Cámara de Diputados. H. Congreso de la Unión DEPF/103/2007
MXN: Mexican pesos

1.2 Historical context of healthcare in Mexico

The structural inequalities of healthcare in Mexico have their origin in the foundation of the national health system in 1943 when both IMSS and SSA were created. IMSS was conceived as a response to the increasing healthcare demand of a growing working class that resulted from rapid economic development (Barraza-Lloréns et al., 2002). IMSS provided workers with social security benefits including healthcare and financial security for retirement, disability or death. Affiliation was mandatory for formal-sector workers and their families. However, informal workers could enrol voluntarily. The Ministry of Health (SSA) was created in the same year to provide public health, sanitation, and to allocate resources to provide health care to those who were uninsured. It was believed that over time, with rapid industrialisation, formal employment would increase along with social security coverage and that government services for the uninsured would only de required for a small fraction of the population (Nigenda et al., 2015).

However, access to social security did not grow at the expected rate. Despite a few decades of sustained economic growth, labour markets did not formalise at the expected rate, nor did social security coverage increase (Flamand and Jaimes, 2015). Moreover, the financial crises in the 1970s and 80s resulted in an increase of informal employment creating a larger proportion of the population who were reliant on the social assistance services. The social security model based on labour status was insufficient to expand healthcare coverage and distribute its benefits equitably. This large gap in healthcare coverage deepened health and socioeconomic inequalities as access was not determined by need but by labour supply status. Those the informal sector of the economy were therefore doubly disadvantaged: both earning lower income and being unable to access social security benefits.

As a response to the increasing gap in access to high quality health services, in 1982, the government started a series of reforms to decentralise health services. In 1983, the Mexican Constitution was revised to include access to healthcare as a citizen's right, rather than merely a labour benefit[4]. However, by the year 2000, 58% of health financing came from out-of-pocket health expenditures (Gómez-Dantés and Ortiz, 2005). This situation imposed a greater burden on poorer households. Furthermore, families were exposed to the risk of catastrophic healthcare expenditures – i.e. healthcare expenditures that exceed a reasonable proportion of household income or total expenditure in a year, typically over 20% of total income; or even impoverishing healthcare expenditures – i.e. expenditures sufficiently large enough to push a household below the poverty line (Wagstaff and van Doorslaer, 2003). Around 3 million families had catastrophic expenditures in 2004; and, in addition to the regressivity of private payments, government expenditure was regressive as well: public resources were higher for the insured families than for the uninsured (Gómez-Dantés et al., 2004). (See Table 1.1)

The prevailing situation where half of the population was not entitled to high quality and effective healthcare was not compatible with the constitutional reform of 1983. To implement this universal human right, it was necessary to introduce a new mechanism to expand coverage to the non-salaried workers and their families, to uncouple healthcare from labour status (Flamand and Jaimes, 2015). *Seguro Popular* originated in that context as a mechanism to provide resources for states and highly specialised hospitals to reduce health access inequities for the unprotected population. The goal was to delegate financial decisions for healthcare spending to local authorities, proximal to where health services were needed. The intention was that resources per SP affiliate would eventually match the resources per social security affiliate to guarantee the same quality standards. Moreover, in order to tackle inequity in resource allocation, the budget would depend on the number of affiliates by state, such that states with more affiliates would receive more resources. Overall, the result would be to reduce the role of political factors in health financing decisions.

[4]The WHO Constitution included the right to healthcare as a human right since 1946. https://www.who.int/news-room/fact-sheets/detail/human-rights-and-health
The UN included the right to healthcare as human right in the Universal Human Rights Declaration, 1948. https://www.un.org/en/universal-declaration-human-rights/

Seguro Popular started with a pilot phase in 2002-2003 and was formally included in the *Ley General de Salud*[5] (Healthcare General Law) in 2004. SP would aim to progressively cover more than 50 million uninsured Mexicans, who were about half of the total population, over a period of 7 years. This new general bill on health that established the social protection system was approved by Congress in April 2003. This law guaranteed access to almost 100 interventions that cover 90% of outpatient services and 70% of inpatient interventions with no co-payment: Mexican families not covered by formal social security programs would receive services free of charge.

1.3 Implementation of *Seguro Popular*

Seguro Popular was specifically targeted to the informal, poor, indigenous poor, and uninsured workers with a goal of achieving universal health insurance coverage (although in a two-tiered system) and reducing catastrophic spending on health. Previously, SSA charged fees below full cost depending on the declared income of the user. SP is financed by both state and federal taxes and beneficiary families excluding the two lower deciles of income. However, virtually no family pays to become affiliated. Since states receive funding for every affiliated family, they have a strong incentive to enrol as many families as possible.

Government healthcare expenditures increased by 85% with the introduction of *Seguro Popular* (Flamand and Jaimes, 2015). *Seguro Popular*'s budget in that same year was over ten times that in 2004, going from 3.4 billion pesos (301 million USD)[6] to 71.2 billion pesos (3.7 billion USD)[7] in nominal terms. Affiliation also grew over ten times from 5.3 million people in 2004 to 53.5 million by 2019. In the same period, the share of out-of-pocket expenditures decreased from 60% of the total healthcare expenditure (Wirtz et al., 2012) to 40% in 2019[8]. To increase the equity of public health financing, beneficiary families are entitled by law to the value in money of the healthcare received by families with formal social security benefits (such as IMSS services).

The insurance consists of four different modules of defined services that are administered free of charge. The first two, which have been in place since the start of the programme, are the *Catálogo Universal de Servicios de Salud* (CAUSES) and

[5] Ley General de Salud. http://www.ordenjuridico.gob.mx/Documentos/Federal/pdf/wo11037.pdf

[6] The exchange rate in 2004 was 11.29 MXN per USD and in 2019 1 USD equalled to 19.25 MXN

[7] Presupuesto de Egresos de la Federación http://www.transparencia.seguro-popular.gob.mx/index.php/transparencia-focalizada/11-pef/54-presupuesto-de-egresos-de-la-federacion-pef-2019

[8] OECD. Health at a glance. Mexico, 2019. https://www.oecd.org/mexico/health-at-a-glance-mexico-EN.pdf

the *Fondo de Protección contra Gastos Catastróficos* (FPGC). The former defines the principal set of covered interventions and medicines. At the programme's start in 2004 it covered 91 essential health interventions and the 142 medications associated with them (Ruvalcaba and Vargas, 2010). These covered over 90 percent of the disease burden in Mexico (Popular, 2007). By 2012, coverage had been expanded to 284 interventions and 391 medications, covering 95 percent of the disease burden (Knaul et al., 2012) and extended coverage to 60 million people. The FPGC is a trust fund, jointly financed by the federal and state governments, that covers costly interventions which may result in catastrophic outcomes (death or bankruptcy, or both) for the household affected. In 2012, the FPGC covered 58 interventions in areas such as cancer treatment, cardiovascular problems or organ transplants.

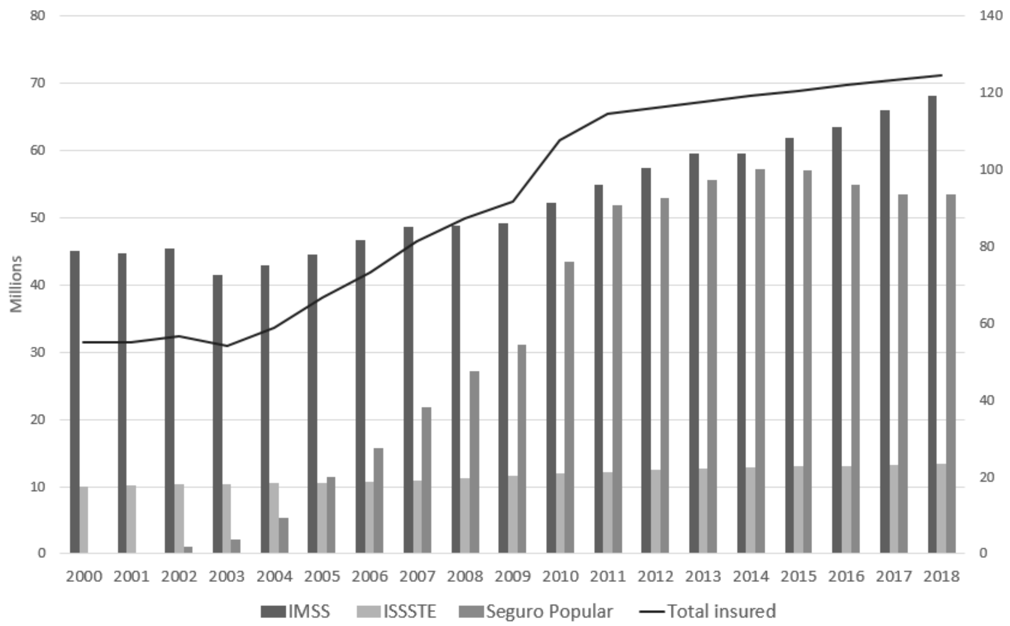

Fig. 1.1 Timing of *Seguro Popular*'s implementation

SP roll-out was completed by 2012 after 52.6 million people had been enrolled (Figure 1.1). The roll-out occurred in stages at both the geographic and individual level. The program was intended to reach areas with greatest need first, but in practice the roll-out was arbitrary, determined mainly by logistical and political factors, rather than health need. Fortunately for the purpose of evaluation, the nature of the roll-out created the conditions for a natural experiment. Implementation began in 2002 with a pilot in 26 municipalities from 5 states and full roll-out started in 2004. The actual geographic roll-out happened as follows: out of 32 states, 5 joined the program for the pilot in 2002[9], 15 additional states joined by the end of

[9]Aguascalientes, Campeche, Colima, Jalisco and Tabasco.

2002[10], six more additional states joined before the end of the pilot in 2003[11] and from the remaining six, three joined in 2004[12] and the last three in 2005[13] (Conti and Ginja, 2020).

SP affiliation within states was primarily targeted to the poorer indigenous population in rural areas and the poorest quintile. Geographically, at the state level, the roll-out was a result of negotiations with the federal government; and each state decided when to enrol their citizens. Geographical roll-out was completed by 2006, and by 2012, with 52 million affiliates (Knaul et al., 2012) population roll-out was finalised (Fig. 1.1).

Each year, the state and federal governments negotiated the target number of households the state would enrol in each quarter of the following year until states reached their official final target (theoretically equal to the number of uninsured households in their state). Officially, the states that were primarily targeted were those with: low insurance coverage, high numbers of uninsured individuals in the first six income deciles, the ability to provide services, high demand for enrolment, and a sufficient budget (Conti and Ginja, 2020). Within states, *Seguro Popular* was rolled out at the municipality level, meaning that a municipality could begin affiliating enrolees once their health care facilities met minimum infrastructure and human resource requirements (Knox, 2008). However, some affiliated municipalities met the minimum requirements in terms of healthcare facilities and personnel while others did not (Gakidou et al., 2006).

The timing in the roll-out of *Seguro Popular* has been used by other studies as an instrument for program affiliation (Galárraga et al., 2010; Sosa-Rubi et al., 2009). The programme was designed to be targeted to the poor and indigenous (Knaul et al., 2005), but previous studies have found that this was not necessarily the case. Roll-out may have been influenced by political factors as well; for example small states can achieve full coverage quickly and this can be used to win votes during election times (Barros et al., 2008). Bosch et al. (2012) determined that the roll out was essentially random, while Barros et al. (2008) and Knox (2008) discuss the political motivations behind the timing of affiliation. Turrini et al. (2016) argue that the roll out was not random and that the municipalities that received access to SP first were more highly educated, had higher expenditures, and higher rates of formal employment.

[10]Baja California, Chiapas, Coahuila, Guanajuato, Guerrero, Hidalgo, México, Morelos, Oaxaca, Quintana Roo, San Luis Potosí, Sinaloa, Sonora, Tamaulipas and Zacatecas.

[11]Baja California Sur, Michoacán, Puebla, Tlaxcala, Veracruz and Yucatán.

[12]Nayarit, Nuevo León and Querétaro.

[13]Chihuahua, Distrito Federal and Durango.

The decentralisation process that resulted in SP had many risks; one was the possibility of corruption from local authorities. Indeed, the decision for SP cancellation was based more on cases of corruption in local governments rather than due to inadequate performance of the programme (Reich, 2020). The newly created INSABI that replaced *Seguro Popular* in January 2020, is a health system with the aim of matching the benefits of the insured and uninsured by centralising funding and establishing the federal government as the only funder and provider. However, there are not explicit rules for allocating financial resources or information on sources of the additional financing needed (Reyes-Morales et al., 2020). The main drivers of the implementation of INSABI were political and ideological principles, rather than the result of deep analysis, planning and evidence from past evaluations (Reich, 2020).

1.4 Previous evaluations of *Seguro Popular*

Impact evaluations inform the effectiveness of public policies and should be an important source of information when deciding to introduce, enhance or cancel programmes. Undeniably, there are political factors that would determine if a policy would ever be implemented. In the case of Mexico, the prevailing inequality in access and outcomes made necessary a deep reform and whether or not *Seguro Popular* effectively addressed the problem is an evaluation question.

SP has been evaluated in other studies. A few notable examples are described here. The Ministry of Health and the independent National Institute of Public Health commissioned a team from Harvard University to conduct an external and independent evaluation of SP. The evaluation team, led by King et al. (2009, 2007), conducted a randomised study at the time of implementation of the programme. They developed an approach to conduct large-scale randomised public policy experiments that are robust to political interventions. They defined 12,824 contiguous geographic regions as health clusters including an actual or future health clinic and the population catchment area around it. From the 32 states, 13 participated in the evaluation with 7,078 health clusters. For the study, 74 pairs from seven states were selected. These 148 clusters included 1,380 localities, approximately 118,569 households and 534,457 individuals. From each pair, they randomly selected one health cluster to receive encouragement for individuals to affiliate with SP and everything needed to implement the programme (health facility, drugs, doctors, etc.) while the other health cluster in the pair did not receive anything additional. They conducted a baseline household survey at the time of randomisation, and follow-ups ten months after and repeatedly at other intervals. To avoid any bias due to the loss of information, instead of the classical randomisation, they use matched pair randomisation, which consists in first selecting pairs of health clusters that are matched on a large set of characteristics, and then, randomly choosing one cluster from the pair to receive treatment. This design provides better balance: on the pair level by matching on the selected variables and on average by randomisation on the variables that were not selected. More importantly, it protects the design from the loss of clusters by finding a pair from the other clusters or deleting the remaining pair. The third part of the process was a parametric adjustment to estimate the quantities of interest. Instead of a difference in means, they would adjust parametrically adding pretreatment covariates, including remaining confounders and interactions. Their design produces unbiased inference even if two of the three steps in their procedure suffers any disruptions. Results from the study showed a reduction in catastrophic payments but no effects on health outcomes or utilisation. Moreover, he study only allows for inference in the study areas or similar communities. According to the authors, generalisation of results would have needed further experiments in different areas in subsequent exercises.

Gakidou et al. (2006) found that catastrophic expenditures are lower for SP insured people. They analysed several outcomes and their evolution in the early years of *Seguro Popular*. Their results show positive results of SP; however, they do not find causal impacts and only show associations. The authors found that around 16-18% of total population without social security or private insurance enrolled in *Seguro Popular* by the first quarter of 2006. The largest fraction of those affiliates were from the two lowest income deciles in communities with medium and low levels of development. The authors found an association between private expenditure in healthcare and enrolment. Expenditure increased from 2000 to 2003, but declined after *Seguro Popular* enrolment more than doubled in 2005. The number of nurses and doctors employed by the Ministry of Health per 1000 population increased from 0.83 to 0.94 between 2001 and 2005. *Seguro Popular* enrolees were found to have a higher probability of healthcare use than uninsured individuals conditional on perceived need. Using a multivariate random-effects regression model, the authors found that enrolment to SP is associated with an increase of all-cause hospitalisation rates. The authors, using a logistic regression model, estimated levels of effective coverage – defined as the delivery of appropriate high quality interventions to those who need them – and found that the insured population was more effectively covered than the uninsured and the differences were statistically significant. Gakidou and colleagues show a decline in mortality on the SP insured population for all causes combined for the major-cause groups since 2000. However, they note that this trend started before SP roll-out. They analyse the impact of SP on protection against financial risk using logistic regressions and find that the proportion of covered individuals paying for medication was significantly lower than that in uninsured individuals. Moreover, affiliates had 14% lower expenditures in drugs than uninsured people. Finally, logistic regression results show that SP protects against catastrophic payments at the population level.

Sosa-Rubi et al. (2009) evaluated the impact of SP on pregnant women's access to obstetrical services. They analysed data from 3,890 uninsured women who gave birth between 2001 and 2006 using a multinomial discrete choice model of the selection of healthcare provider to give birth between an SP facility, a private health unit or a non-accredited clinic run by the Ministry of Health (MoH). They address the possible endogeneity of the decision to enrol in *Seguro Popular* through instrumental variables. Results show that the women from SP-participant households had a much stronger preference to use a SP-facility than to use a MoH clinic; they also preferred to attend an SP facility rather than to pay out-of-pocket to use a private clinic, but the association was weaker. They found heterogeneity in the treatment effects according to women's education and health, as well as household assets and residence strata. Taking advantage of the gradual introduction of the programme as identification strategy, they use binary indicators for the year (2002, 2003 and 2004) the women's municipality of residence was officially incorporated to *Seguro Popular* as instruments. However, they only looked at outcomes from 2006 in a cross-sectional study and they may have been unable to control for unobservable characteristics in that year. Extending this analysis by adding data from other years would be useful to reduce bias.

In a subsequent study, Harris and Sosa-Rubi (2009) complement the analysis by Sosa-Rubi et al. (2009) by employing a latent class model to assess the impact of *Seguro Popular* on number of prenatal visits in a cross-sectional sample of 4381 women who gave birth between the years 2002 and 2005. They assume that women belong to a latent class according to healthcare choice that can be modelled as a function of observable covariates. They find a positive impact on access for obstetric services on pregnant women; specifically, they show that enrolment in SP was associated with a mean increase in 1.65 prenatal visits during pregnancy. The authors identify that 59% of the effect is due to an increase in prenatal care among women who had little or no access to care. They use an ordered probit model and try to address the endogeneity of the voluntary enrolment to SP. Similarly to Sosa-Rubi et al. (2009), the fact that the analysis is conducted on cross-sectional data from a single year, may not address the endogenity issue. Moreover, the instrumental variable in Harris and Sosa-Rubi (2009), may not satisfy the exogeneity assumption. They use a locality-level variable for the percentage penetration of *Seguro Popular* among the eligible population. The authors claim that penetration depends on investments on healthcare infrastructure made by each state and on their efforts to inform the population about the availability of the programme. Therefore, it is treated as an exogenous variable. However, the level of penetration among eligible population may be affected by individuals factors related with their decisions to enrol and with class membership.

The present study concludes a year after the cancellation of SP, which provides sufficient information to evaluate SP over its whole duration. The cancellation of SP also coincides with the new coronavirus pandemic. By December 2020, there were more than 83 million confirmed cases of COVID-19 and more than 1.8 million deaths worldwide. At that time, Mexico with 123,845 deaths had the fourth highest mortality count by COVID-19 after USA, Brazil and India and the 13^{th} for most confirmed cases. COVID-19 is expected to be, in 2020, the second largest cause of death in the country, overcoming diabetes mellitus, the second mortality cause in 2019 with 101,257 deaths[14]. For the first time in 30 years, an infectious disease is among the top five causes of death. This situation is challenging the capacity of health systems around the world both in developed and in developing countries, further emphasising the critical importance of health systems evaluation towards its optimisation.

1.5 Supply-side considerations of *Seguro Popular*

A policy expansion of this magnitude would be expected to have influence on both the demand- and the supply-side incentives. This study primarily focuses on the demand-side, specifically on the change in enrollees' behaviours. There are, however, supply-side incentives at play. *Seguro Popular* changed the provider payment mechanisms. The funding came from three sources: a contribution from the state, called 'the social fee'; a second contribution came from the federation and the states; the third came from an enrolling fee paid by the affiliated families according to their income level. Around 5/7 of the total is paid by the federal government and the rest by the other actors (Frenk Mora, 2004). The poorer 20% of affiliates are exempted from the enrolment fees. An important change in the financing mechanism was the basis on which the budget was assigned. Previously, resources were allocated according to bureaucracy size: states would receive funds according to installed infrastructure regardless of population served size. The policy was reformed to allocate funds according to affiliated families. The idea was to match resources with needs. The more affiliation, the more funding received. The incentives introduced

[14] The first largest cause of death in 2019 was heart conditions with 149,368 deaths. https://www.inegi.org.mx/contenidos/saladeprensa/boletines/2019/EstSociodemo/Defunciones Registradas2019.pdf

were for states to affiliate as many families as possible. However, states received a fixed amount by affiliated family; therefore there was an incentive for efficiency. Moreover, the mechanism of fund allocation was transparent through an auditable registry of beneficiaries.

The intention for affiliation to be voluntary was to introduce an incentive for providers to offer high quality services. In that way, providers would avoid de-affiliation and not lose any resources. However, in practice, family fees were not required for affiliation, and therefore, families did not have strong incentives to de-affiliate. Families would remain beneficiary and use a combination of *Seguro Popular* and private health services according to their needs and preferences. The policy was designed to increase availability of resources, and to use them efficiently while providing high quality healthcare. There is evidence of increased availability of resources (Celhay et al., 2019; Huffman and van Gameren, 2019). In practice, therefore, the threat of de-affiliation was not credible, thus, it did not introduce an incentive for high quality services.

Changes to incentives have to be taken into account when evaluating a policy like *Seguro Popular*. An evaluation of a health insurance programme for the poor in Colombia (Miller et al., 2009) identifies the change in incentives from both the demand and the supply side introduced by the 'Subsidised Regime' (SR) programme. SR is a health insurance programme targeted to the poor. It differs from *Seguro Popular* in that the enrolees are fully subsidised to purchase insurance from private insurers approved by the government. Insurers must provide standardise benefit packages in exchange for standardised premiums. SR unlike SP operates as a health insurance. Under SR as *Seguro Popular*, providers receive a fixed amount per enrollee to use on primary healthcare. The incentive is in both cases to constrain primary care expenditure. This is the efficiency incentive described above. For specialised services, providers receive a fee for each service they provide in the case of SR. For SP, the resources for the specialised services come from a fund for catastrophic healthcare expenditures on a by service basis as well. In the SR case, the incentive is to provide all reimbursable services; similarly, in the case of SP, the incentive is to provide the services included in the interventions covered by the catastrophic fund.

1.6 The present evaluation of *Seguro Popular*

The second chapter of the thesis is an evaluation of the impacts of *Seguro Popular* on the health status of adults with a focus on hypertension as a known underlying cause of metabolic conditions, such as cardiovascular disease and diabetes, and on the change in utilisation of health services. Using a panel dataset that covers from 2002 to 2012, treatment effects are estimated using difference-in-difference specifications. The third chapter is concerned with the distributional effects of the SP programme. Using both cross-sectional and longitudinal data, the chapter evaluates the effect of the programme on childhood nutritional health. The fourth chapter of the thesis is an assessment of the financial protection mechanism of *Seguro Popular*. The study, using a repeated cross-sectional dataset on households' income and expenditure, estimates the programme's impact on out-of-pocket healthcare payments, catastrophic payments and impoverishment and its antipoverty and distributional effects. Treatment effects are estimated by fixed effects and instrumental variables for average effects; while distributional effects are estimated using non-linear difference-in-differences methods. Given that the policy targets people with no social protection in the lower income deciles it is reasonable to think that a reduction in out-of-pocket payments for healthcare would have positive effect in welfare and we would expect a higher effect on the poorest.

Chapter 2

Impact of *Seguro Popular* on healthcare utilisation and health outcomes

Abstract

Over the last 30 years, the Mexican government started an ambitious expansion of the health system with the aim of granting universality of healthcare coverage for more than 50 million unprotected individuals. *Seguro Popular de Salud*, a health social insurance programme, was introduced with the goal of tackling the prevailing inequality in healthcare access. Exploiting the time and space variation in the introduction of *Seguro Popular*, the present study uses differences-in-differences specifications with variations in identification strategies to estimate the programme's impact on health outcomes with an emphasis on risk factors for adult metabolic conditions, and the effect on utilisation of health services. Results show that the programme had no effects on health outcomes or health care utilisation; however, there is a long-run effect on mortality rates for different age groups. Intention-to-treat effects showed a reduction in the mortality rate of 24 deaths per 1000 births, 14 years after the introduction of *Seguro Popular* among poorer municipalities. Moreover, the impact seems to be increasing in a sustained trend. The policy also had an impact on the mortality rate among children under 10 years of age to a lesser degree.

2.1 Introduction

The World Health Organization (WHO) defines universal coverage as access to effective and high quality healthcare to all people when needed without the risk of financial hardship (WHO, 2010). WHO (2005) defines the necessary actions to attain universal coverage. These are, among others, that health systems financing must allow for a method of healthcare prepayment to spread the risk across the population to prevent catastrophic and impoverishing health payments for households; to ensure the equitable distribution of good quality infrastructure and human resources to provide in turn equitable and good-quality healthcare; to develop a sustainable financing mechanism for the health system; and to meet the healthcare needs of the population, improve its quality, and to reduce poverty[1].

[1] https://www.who.int/health_financing/documents/cov-wharesolution5833/en/

According to the Mexican government, with the enrolment of over 50 million people into *Seguro Popular*, Mexico in 2012 had attained universality of access to healthcare with a comprehensive healthcare package and financial protection (Reich, 2020). While evidence suggests that there are still people with no healthcare coverage[2], catastrophic healthcare expenditures (King et al., 2009; Knaul et al., 2005; Sosa-Rubí et al., 2011), as well as impoverishment (Knaul et al., 2018) were reduced. Importantly, the challenge remained to ensure to provide more equitable and high quality health services across the population. The aim of this study is to evaluate the progress in universalisation of healthcare coverage in the Mexican health system with the introduction of *Seguro Popular* in terms of improving health status and healthcare utilisation.

2.2 Background

2.3 The impact of universal health insurance in other countries

High costs are one of the main barriers to accessing healthcare. Health insurance seeks to overcome this barrier by managing funds through risk-pooling mechanisms, so that the burden of cost of a treatment does not solely fall on the individual receiving it, but is shared across a group of individuals. In this kind of system access is expanded and individuals are financially protected. There is an increasing interest for countries to reach universal healthcare coverage. Moreno-Serra and Smith (2012) conducted a literature review on the impact of universal healthcare coverage, and found that universal healthcare coverage improves citizens' health and brings economic benefits through expanding access to health services and protecting against out-of-pocket and catastrophic expenditures. Moreover, findings suggest that the benefits of universal coverage will be larger for less healthy and more financially vulnerable individuals.

In a systematic review, on the impact of social health insurance in low- and middle- income countries, Acharya et al. (2013) found that uptake of services is less than expected; they also found no strong evidence of impact on utilisation or health status. However, there were, in some cases, some impacts on financial risk protection from out-of-pocket expenditures but the effect on the poor was weaker compared to the general population. They found that enrolment rates varied across programmes

[2]CONEVAL. Evolución de las dimensiones de pobreza 1990-2018. https://www.coneval.org.mx/Medicion/Paginas/Evolucion-de-las-dimensiones-de-pobreza-.aspx

and were determined by many factors. In some cases, female-headed households, elderly-headed households, family size and education attainment had positive effects on enrolment; while initial health conditions or distance to health centres did not show an effect. On utilisation, they did not find a higher probability of care seeking resulting from enrolling. Moreover, they did find different results for the same insurance programme in the literature. From 15 studies reporting utilisation, they found that 9 reported a higher utilisation rate among the insured. However, they note that the increase does not translate necessarily to improved welfare as it could be the result of moral hazard. On financial protection, the authors found that only 4 out of 16 studies reported conclusive indications of lower average OOP expenditures for the insured. They found seven studies reporting mixed results of insurance on financial protection and two showed no effect. Five studies reported lower incidence of catastrophic healthcare expenditures. From the literature reviewed, Acharya et al. (2013), only found that six studies reported on health outcomes and among those studies only two reported an improvement in health of covered population. They explain that if there is not an improvement on quality of services, utilisation will not grow and health status will not improve.

In a more recent systematic review, Erlangga et al. (2019) reported findings of public health insurance impact on healthcare utilisation, financial protection and health status from 68 studies published between 2010 and 2016. Overall, having health insurance was found to increase healthcare utilisation. The effect of utilisation on curative care was found to be positive in 30 out of 38 studies. Preventive care was found to have a less clear effect: only 4 out of 7 studies reported a positive effect, two studies reported a negative effect and one did not find any effects. Among a subsample of studies that controlled for biases more rigorously, seven studies reported a positive relationship between insurance and utilisation; one reported no effect; and one study reported a negative impact. Financial protection findings were less clear, although overall they seem favourable. From the 34 studies that reported on the effect of insurance out-of-pocket healthcare expenditure, 17 found a reduction on health expenditures, 15 found no statistically significant effects and two studies found that insurance increased out-of-pocket expenditure. From the 14 studies reporting an effect on catastrophic healthcare expenditures, nine reported a reduction on the risk of incurring catastrophic expenditures, three found no effect and two found that insurance increase the risk. Two studies of the sample reported no effect on impoverishment and four studies reported no effect on non-healthcare consumption. There were limited results of the relationship between public health insurance and health outcomes. However, there was some evidence of a health improvement impact of the insurance. From 12 studies reporting health effects, 9 reported a positive effect, one found a negative effect and two found no effect. Erlangga et al. (2019), conclude that public health insurance seems to have an effect on utilisation, no clear effect on financial protection and a promising effect on health status, but that further research is needed.

Results from the RAND Health Insurance Experiment, an experimental study conducted in the United States from 1974 to 1982, that randomly assigned people to different insurance healthcare plans, showed that 87% of the participants of the free care plan used any service vs only 68% of the participants on the 95% co-payment plan (Manning et al., 1987). Nevertheless, it had limited effects on health outcomes (Newhouse et al., 1993). More recently, evidence from the Oregon Health Insurance Experiment in 2008, which randomly gave uninsured low-income adults the opportunity to apply for Medicaid, one year after the lottery, showed that the treatment group had higher healthcare utilisation, lower out-of-pocket medical expenditures and better self-reported physical and mental health than the control group (Finkelstein et al., 2012).

There is also growing literature on the effects of healthcare coverage expansion in developing countries. In a large panel dataset for 153 countries, Moreno-Serra and Smith (2015) showed that expanded health coverage by increases in public spending results in lower child mortality with larger effects observed in lower-income countries. Thailand introduced an equitable universal healthcare coverage system in 2001 that reduced the incidence and intensity of catastrophic and impoverishing out-of-pocket healthcare payments (Somkotra and Lagrada, 2008); and reduced post-neonatal mortality and out-of-pocket expenditure (Gruber et al., 2014).

Latin American countries, since the 1990s, have been strengthening health systems towards a universal health coverage to provide financial protection, reduce inequalities in health and access, and improve health outcomes. The aim has been the expansion of services especially to the poor and uninsured population. Most of these countries have complemented these reforms with a complete package of benefits to alleviate poverty and social inequalities. Brazil and Cuba, for example, introduced tax-financed universal healthcare systems (Atun et al., 2015). The universal health insurance in Colombia increased access and utilisation of healthcare and reduced the incidence catastrophic health expenditures especially for the most vulnerable people (Giedion and Uribe, 2009). In Argentina, *Plan Nacer*, an insurance programme that offers reproductive and child healthcare to uninsured families, increased the use and quality of healthcare. Moreover, it had an impact on health status by reducing the probability of low birthweight and the risk of in-hospital mortality (Gertler et al., 2014).

2.3.1 The epidemiological context in Mexico

A rise in life expectancy, together with the decline in childhood mortality and general mortality, has led to a change in disease profile of the Mexican population. In the past three decades, infectious diseases have no longer been among the main mortality causes in Mexico, but have been over-taken by non-communicable diseases and injuries (Soto-Estrada et al., 2016). In the mid-twentieth century, 50% of deaths were due to common infections, maternal and infant mortality and diseases related to malnutrition. The infant mortality rate is currently 12 deaths per 1000 births (Soto-Estrada et al., 2016), which represents a 50% reduction from 2000 (OECD, 2016). By 2010, these illnesses account for less than 15% of deaths in the country, while non-communicable diseases and injuries represent 75% and 11% of deaths, respectively (Dantés et al., 2011). Metabolic and chronic conditions are of particular importance. The leading causes of death are heart disease and diabetes mellitus. From 2000 to 2012, the prevalence of overweight in adults rose from 62% to 72.5%; 37% children are overweight or obese, while 32% of adults are obese (OECD, 2016). Prevalence of diabetes is above 15%, greater than the average among OECD countries, which is 6.94%. Despite its epidemiologic transition, Mexico still has marginalised populations where infectious and maternal and infant mortality are the main causes of death (Soto-Estrada et al., 2016).

Public investment in healthcare in Mexico has increased from 2.4% to 3.2% of GDP in the last ten years; in the same period *Seguro Popular* has covered more than 50 million people. Out-of-pocket expenditures have reduced noticeably, from 60% (Wirtz et al., 2012) to 41%[3] and impoverishing expenditures, previously prevalent at 3.3% of the population, are currently only about 0.8% (OECD, 2016). However, the proportion of health budget assigned to administrative expenditure is at 10%, the highest in the OECD. Health expenditures account for 5.5% of GDP, while the OECD average is 8.8% (OECD, 2016).

2.3.2 Contribution to the literature

Evidence suggests that *Seguro Popular*'s impacts on health outcomes varied with the timing of introduction. Early studies found that *Seguro Popular* had from non-existent in most cases to very mild treatment effects. However, more recent evaluations have found positive impacts on health of beneficiaries. For example, a randomised study was designed and implemented with cooperation from the Mexican government when SP was first introduced to estimate its impacts on health care

[3] Organisation for Economic Cooperation and Development. Health at a Glance 2019: Mexico. https://www.oecd.org/mexico/health-at-a-glance-mexico-EN.pdf

utilisation, health, and financial protection ten months after programme introduction (King et al., 2009, 2007). At the outset of this study, the authors claimed that ten months would be ample time to measure changes in utilisation due to the programme. Indeed, the authors found a decline in 23% in out-of-pocket healthcare payments. However, the study found no effects of the programme on health outcomes or healthcare utilisation. According to the authors, as the experiment was run in a small number of arbitrarily selected municipalities, it may suffer from external validity[4] concerns common to randomised controlled trials (Deaton and Cartwright, 2018); and by being assessed ten months after implemented, this was arguably not sufficient time for treatment effects on the health outcomes studied to be observed.

Barros et al. (2008) used a triple differences approach similar to the current study, that, in addition to group and time dimensions, includes an intensity parameter that indicates the degree of progress in individual affiliation with respect to target affiliation by municipality. The intensity variable in the current study, in contrast, is defined with respect to the maximum affiliation reached by municipality, which is a more realistic measure. Moreover, the dataset used in Barros et al. (2008) is two waves of the repeated cross sectional survey *Encuesta Nacional de Salud y Nutrición*. Even though it is a representative survey, the estimates obtained may be biased as the surveyed households are different across years. The strategy of the current study is to take advantage of the longitudinal structure of the Mexican Family Life Survey following individuals over ten years in three waves, which improves the estimations.

Pfutze (2014) found that *Seguro Popular* reduced infant mortality rate by 5 in 1000 births after five years of implementation. However, that study used only cross sectional data and, therefore, does not address the bias caused by the endogenous affiliation completely. Moreover, the census data, as the author notes, misses some information on births and deaths. In contrast, the present study examines infant mortality looking at mortality rates from 2000 to 2017 to capture short and long-run effects using the dataset of all death certificates, which is not affected by recall bias. Turrini et al. (2016) found a mild increase in height-for-age in children after the programme has been established for several years. However, they compare different children at different times. In contrast, the current study follows the same individuals across three waves over 10 years, which improves estimations.

Pfutze (2015) finds evidence of reduced risk of a miscarriage of 0.04% for each percentage point increase in coverage over the 2004-2008 period. However, it relies on cross sectional data and considers affiliation to *Seguro Popular* starting in 2004, which ignores the two years pilot phase, which could bias the results. Conti and Ginja (2020) find a significant and clear impact of SP on health outcomes: according to

[4]External validity refers to how generalisable are the result of a study in a different context.

their results, child mortality was reduced by 10% in poor municipalities. They exploit the staggered timing of implementation of SP by comparing changes in mortality rates for all age groups in municipalities that introduced it in different years between 2002 and 2010 and found a reduction in mortality rates among infants living in poor municipalities. They also found that the introduction of *Seguro Popular* in poor municipalities is associated with an increase in healthcare utilisation. Obstetric-related hospital admissions had an immediate increase of 7% and with a 6% increase in general hospital admissions. However, their analysis covers the first ten years of the programme, while the present study extends the scope through 2017 to capture short and long-term effects.

Given that the number of diseases, treatments, and medications covered by *Seguro Popular* increased drastically between 2004 and 2012 (Knaul et al., 2012), it is reasonable to think that utilisation of the programme may have increased as the programme catalogue expanded and affiliates learned more about the improvements. In fact, after the early experiment, most of studies found a positive impact of *Seguro Popular* on utilisation. Gakidou et al. (2006) found that SP affiliates used more inpatient and outpatient services than uninsured people; while Scott (2006) found higher utilisation rates of public health services for SP affiliates than for the rest of the uninsured. King et al. (2009) found no effect on utilisation. Sosa-Rubi et al. (2009) found a positive impact of SP on access to obstetrical services. Harris and Sosa-Rubi (2009) found that SP affiliation was associated with an increase by 1.65 prenatal visits during pregnancy. Knox (2016) also found an increase in use of medical care. In another study using the original experimental data complemented with administrative data, Spenkuch (2012) finds that programme affiliates are less likely to use preventive health care services, a finding that he takes to be evidence of moral hazard among beneficiaries. On the other hand, there is growing evidence that *Seguro Popular* does reduce out-of-pocket health care expenditure (Barros et al., 2008; Galárraga et al., 2010; Sosa-Rubí et al., 2011).

Most of the research on *Seguro Popular* on the impact of the programme on health outcomes, use of health care services, and household spending on health care have relied on cross-sectional data and cannot completely control for bias due to self-selection into *Seguro Popular*[5]. Only a few studies have used panel data to measure the causal impact of *Seguro Popular* on health outcomes using the geographic roll out of the programme as a natural experiment: two of them look at early years of the programme (Barros et al., 2008; Knox, 2008); one focuses only on urban areas (Knox, 2016); one looks at children health outcomes (Turrini et al., 2016); and another looks

[5]Individuals that enrol in *Seguro Popular* expect a higher benefit than individuals that choose not to enrol.

at the municipality-aggregated data (Conti and Ginja, 2020). In this study, the natural variation of implementation timing on the municipality level will be used to estimate the impact of *Seguro Popular* on health outcomes and utilisation among a panel of adults.

2.4 Objective

The main function of public health insurance is to protect individuals from financial risk caused by health shocks and allow them to smooth consumption over time. People in the lower income groups and those outside the formal economic sector are particularly vulnerable to financial hardship caused by paying for healthcare out-of-pocket. In low- and middle-income countries, governments aim for universal healthcare coverage through extending the social health insurance that mainly covers the formal sector to the informal sector. This is funded through taxes and generally offering a reduced package (Acharya et al., 2013). Another alternative to provide financial protection to the poor is through subsidised voluntary household enrolment schemes offering a pre-defined benefit package. These schemes are completely subsidised or charge a premium well below the actuarial fair price (Acharya et al., 2013).

Selection into insurance may introduce bias in the evaluation results. One way is through adverse selection. Individuals who voluntarily enrol might have a higher probability of needing the services than those who do not, and those differences in need may be highly associated to health outcomes. Therefore, a comparison of the health outcomes between voluntarily insured and the uninsured will result in biased estimation. The impact of the programme may be underestimated because on average the affiliated will have lower levels of health. In addition to this problem, it is possible that the programme could be partly responsible for the rise in certain conditions, especially those that are metabolic and chronic as a result of moral hazard. Moral hazard is present in public health insurance when covered individuals do not use preventive care or even neglect their health since the price of treating illness has fallen. The presence of moral hazard complicates the estimation of programme effects. In this case, increases in utilisation may be observed, but with a limited or even negative impact on the health of the insured. Measuring preventive health utilisation and health outcomes can provide insight into whether the insured are engaging in moral hazard (Spenkuch, 2012).

A way to reduce these types of biases is to assign the programme randomly among the population. In a randomised evaluation design approach, any change in the outcomes can be attributed to the programme and the likelihood of confounding by these factors will reduced. In observational or non-experimental frameworks, which are more common in health policy research, assignment of individuals to treatment is not randomised but based on population characteristics that can be observed or unobserved. The alternative in these contexts, is to identify a source of exogenous variation in the assignment to treatment that is independent to the outcomes in question to reduce the bias of estimates (Jones and Rice, 2011).

Following the definition of universal healthcare coverage given by WHO (2010), the aim of this study is to assess the degree of success *Seguro Popular* achieved in creating universality of healthcare coverage in Mexico in terms of access to healthcare and health status of affiliates. Health outcomes are divided into general health measurements and self-reported health status. The health variables of interest for this study are related to metabolic diseases and chronic conditions. Previous evaluations of SP focus on child health because health status in young ages is more reactive to health insurance programmes and therefore effects are easier to detect (Conti and Ginja, 2020; Pfutze, 2014; Turrini et al., 2016). However, given the global epidemiological transition, where infectious diseases (more commonly prevalent in young children) are no longer among the principal causes of death, and chronic conditions (more prevalent among adults) and accidents are more prominent in mortality causes, the focus for assessing the programme effects should also be on these types of conditions. While the declining importance of infectious diseases in comparison to metabolic and chronic conditions in terms of mortality and morbidity is a common trend in the world (Murray et al., 2015), it is important to assess the contribution of *Seguro Popular* to this transition.

2.5 Data

To evaluate improved access to healthcare and health outcomes, the data are taken from Mexican Family Life Survey (MxFLS) (Rubalcava and Teruel, 2006, 2013), a panel survey that spans the temporal and spatial roll-out of SP. The first wave (MxFLS1) was conducted in 2002, before the official SP implementation; the second (MxFLS2) occurred in 2005-2006, one year after implementing; and the third (MxFLS3) was conducted in 2009-2012. The baseline sample was representative at the regional, rural, urban, and national levels. It contained a sample of 35,677 individuals in 8,440 households, located in 150 communities spread over 16 states.

MxFLS includes extensive information on health-related outcomes including self-reported health, illness, use of health care, healthcare expenditures, mental health, maternal health and infant health outcomes as well as anthropometric measures and biomarkers including cognitive ability. The survey contains detailed information on socioeconomic status at the household level and on the individual level. MxFLS also included information on social programmes; for each household member, of at least 14 years old, the survey collects information on whether household members received benefits from social programmes and if they are covered by health insurance (and if so, from what source). The information on individual enrolment to SP comes from this survey. The survey also includes information on the availability of health care services within a municipality.

Additionally, administrative datasets were consulted. To estimate effects on mortality rates by age groups, administrative data on population, deaths and births are utilised to construct death rates by municipality for the years from 2000 to 2018. The data comes from the National Population Council (CONAPO)[6] and the National Institute of Statistics and Geography (INEGI) from Mexico[7]. The information on municipality-level enrolment comes from an administrative dataset of the total of individuals affiliated to the programme at the end of the year from 2002 to 2019. This data is used to construct an indicator of the roll-out at the municipality level. Related literature considers *Seguro Popular* to be introduced to a municipality once 10 families or more are affiliated (Bosch and Campos-Vazquez, 2014; Conti and Ginja, 2020; Del Valle, 2014). As the data used in this study only specifies the number of individuals and not family membership, the threshold used here is 40 individuals, defining an average household to be composed by 4 individuals. Results do not change significantly when this specification is altered.

In this study, the effects of *Seguro Popular* are estimated on four types of outcomes: mortality rates, health measure outcomes, self-report health outcomes and healthcare utilisation. Impacts on mortality rates are estimated for infants, children from 1 to 10 years old, adults of 20 years old and older adults of 60 and above.

The health measure outcome variables considered are high blood pressure and anaemia. These conditions are representative of the disease profile in Mexico, where given the high prevalence of overweight and obesity, metabolic conditions as diabetes and cardiac ailments are the main causes of death (OECD, 2016); and at the same time, infectious diseases and malnutrition-related conditions are still prevalent among poorer population (OECD, 2016). High blood pressure has remained constant in Mexico during the years analysed in this study. In 2000, hypertension prevalence was 30.1% and in 2012 it was 31.4%(Campos-Nonato et al., 2013). In contrast, the prevalence of other metabolic conditions as obesity and diabetes has worsened in

[6] Proyecciones de la Población de México y de las Entidades Federativas, 2016-2050 y Conciliación Demográfica de México, 1950-2015. CONAPO. https://www.gob.mx/conapo/acciones-y-programas/conciliacion-demografica-de-mexico-1950-2015-y-proyecciones-de-la-poblacion-de-mexico-y-de-las-entidades-federativas-2016-2050

[7] Mortalidad general. INEGI. https://www.inegi.org.mx/programas/mortalidad/?ps=microdatos

those years. Overweight and obesity prevalence had a statistically significant increase of almost 10 percent points growing from 61.8% in 2000 to 71.3% in 2012 (Barquera et al., 2013). The hypothesis of this study is that the apparent no change in high blood pressure may be the result of opportune detection or treatment caused by an improved access to healthcare of *Seguro Popular* enrolled families.

Anaemia is a nutritional condition caused by insufficient iron intake. The prevalence in Mexico in 2012 was 11.5% among non-pregnant women and 17.9% among pregnant women (Shamah-Levy et al., 2013). Anaemia prevalence declined between 2000 and 2012 by 10 percent points among non-pregnant women, and by 13.5 p.p. among pregnant women. The reduction in prevalence observed may have been caused by improved nutrition in women in general or by an opportune detection and appropriate treatment. Health interventions may reduce iron deficiency by prescribing iron supplements (Shamah-Levy et al., 2013). The objective of this work is to examine if there is an impact of *Seguro Popular* in that reduction. Moreover, there is evidence of increased utilisation of obstetric services among *Seguro Popular* recipients (Harris and Sosa-Rubi, 2009; Sosa-Rubi et al., 2009) this may have had an impact on anaemia detection and treatment among pregnant women.

The self-report health outcomes analysed are individuals' assessment of current and past health. The healthcare utilisation variables included are outpatient healthcare visits in the last 4 weeks, inpatient visits in the last 12 months and health insurance coverage status.

Finally, the socio-economic and demographic covariates included are, on the individual level: age, sex, civil status, and years of education; on the household level, household size, income level and whether if any of the household members received benefits from *Oportunidades*, a conditional cash transfer programme; and a marginalisation index on the municipality level.

2.6 Methods

The main hypothesis of this study is that the healthcare coverage expansion introduced by *Seguro Popular* will increase utilisation by reducing its cost, which will result in improved population health.

Identification strategy

This study uses two definitions of treated population to test the hypothesis depending on the enrolment level. The main strategy is to use the municipality-level affiliation. People that decide to enrol in the programme may have different unobservable characteristics than people that decide not to enrol. Therefore, simply comparing outcomes of the affiliated to the non-affiliated will give biased estimations. To account for those differences, the treated population is defined as individuals residing in municipalities where the programme has already been roll-out regardless of their affiliation status. Comparing outcomes of people according to municipality level affiliation rather than individual voluntary affiliation reduces the self-selection bias introduced by the differences between individuals who decide to enrol and individuals that do not. Therefore, the parameters estimated are intent-to-treat effects on the treated. An alternative strategy uses individual level affiliation to find average treatment effects on the treated. People who report being enrolled in the programme are compared with the eligible population that reported not being enrolled. To account for the possible bias, treatment and control individuals are matched at the baseline sample before implement a difference-in-differences (DD) estimation. However, the approach using municipality-level identification to account for selection bias is preferred.

A simple comparison between municipalities that are enrolled with those that are not, would be biased as there may be unobserved factors that explain enrolment. By comparing before-and-after outcomes in the treated municipalities, a DD design controls for observed and unobserved factors in the group that are constant over time; and by comparing the before-and-after outcomes in the untreated municipalities but shared comparable conditions as the participants, bias introduced by time varying factors are accounted for.

The treatment effect is the change in the potential outcome of an individual i after receiving the intervention compared to a control.

$$\Delta_i = y_i^1 - y_i^0 \tag{2.1}$$

where the superscript 1 represents treatment and 0 represents control. Thus, the evaluation problem is a missing data problem, as we never observe y_i^1 and y_i^0 at the same time (Holland, 1986). To solve this problem, the DD design first takes the before-and-after comparison for the treated as

$$(Y_{i1}|T_i = 1) - (Y_{i0}|T_i = 1) \tag{2.2}$$

where Y_{i1} is the potential outcome after treatment; and Y_{i0} is the before treatment potential outcome. T_i is the treatment indicator that denotes treatment if $T = 1$, and $T = 0$ if control. The before-and-after comparison for the control group is given by

$$(Y_{i1}|T_i = 0) - (Y_{i0}|T_i = 0) \tag{2.3}$$

The DD estimator results from taking the difference between the previous two expressions:

$$DD = E[(Y_{i1}|T_i = 1) - (Y_{i0}|T_i = 1)]\check{}E[(Y_{i1}|T_i = 0) - (Y_{i0}|T_i = 0)] \tag{2.4}$$

For DD to be valid, it is necessary that there are no time-varying differences in outcomes between groups which means that changes in outcomes follow parallel trends. This assumption can be tested by looking at the trends followed by treated and comparisons before treatment. The DD estimator in a regression framework is given by:

$$Y_{it} = \tau t_t * T_i + \alpha t_t + \gamma T_i + \varepsilon_{it} \tag{2.5}$$

where $DD = \tau$, α represents the time trend effects, γ the time invariant group effects and ε an error term assumed to be independent and identically distributed.

2.6.1 Intent-to-treat effects

To estimate the treatment effects of SP, the gradual roll-out of the programme is used as the identification strategy. Nationally, SP was implemented at the municipality level starting from 2002 in a pilot stage with 7% of municipalities; 15% in 2003. The official start in the programme was 2004 with approximately 30% of municipalities across the country and continued progressively, reaching more than 99% in 2011 (Fig. 2.1). These numbers differ slightly from the MxFLS sample used here, where individuals are followed on three different survey waves, 2002, 2005-2006 and 2009-2012. In the first wave, 24% of the sample surveyed live in a municipality where the programme was implemented in 2005; by the second wave, 76% of the households live in a municipality where the programme was roll-out in between 2002 and 2006; and by 2009, the whole population lives in a treated municipality and are intention-to-treat cases.

As the programme targets the uninsured population, eligible individuals are those who are not affiliated to social security. By law, an individual protected by any institution of social security cannot be affiliated to *Seguro Popular*[8].

To estimate intention-to-treat effects, the treated population is defined as those individuals living in a municipality where the programme was already implemented at the time of the survey in 2002, 2005 or 2009, and who are eligible for the programme. In Figure 2.2, the continuous line represents the treated population, and the dashed lines the three observation points. All outcomes are observed before and after one

[8] Ley General de Salud. http://www.ordenjuridico.gob.mx/Documentos/Federal/pdf/wo11037.pdf

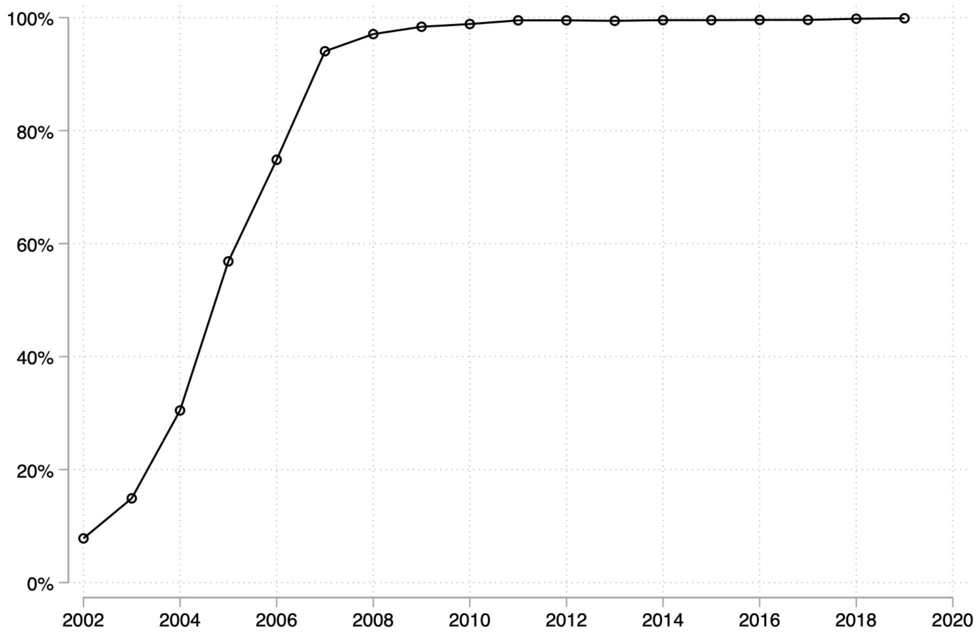

Fig. 2.1 Municipality level roll-out by year

and two years of affiliation for the 2005 wave. For the 2009 wave, outcomes are observed from zero to five years of affiliation. Population who voluntarily enrol to SP are likely to be different from the ones who do not, and probably those differences are related to the outcome of interest. For that reason, intent-to-treat effects are estimated as the difference between eligible and non-eligible population.

The first approach to estimate treatment effects is a difference-in-differences design (DD). The treated group consists of individuals from families that reported not being protected by social security and the control group consists of individuals from families affiliated to social security. The outcomes of treatment and controls are compared before and after treatment. Treatment does not occur at the same time across municipalities, but in different years between 2002 and 2008 in the municipalities included in the survey. The time of treatment is re-centred for all municipalities so that all observations before $t = 0$ belong to the pre-treatment period and all observations after $t = 0$ belong to the post-treatment period (see Table 2.1).

Between the first and the second period, 70% of the sample became treated. Those municipalities have one observation in the pre-treatment period and two observations in the after-treatment period. The 30% that remained untreated in the second wave, are individuals with residence in municipalities where policy adoption takes place after the second wave and before the third wave (see Fig. 2.3). Those municipalities have two observations in the pre-treatment period and one after treatment. Individuals

Fig. 2.2 Municipality level roll-out by year (MxFLS sample)

Table 2.1 Matrix of populations and periods

	Period	
Group	Non-eligible (SS) pre roll-out	Non-eligible (SS) post roll-out
	Eligible (no SS) pre roll-out	Eligible (no SS) post roll-out

Notes. This table shows the population and timing definitions for equations 2.6 and 2.9

who were always-treated: those living in municipalities who became treated in 2002, by the start of the survey, were dropped from this specification. The model used to estimate intention-to-treat effects, compares the outcomes between eligible and non-eligible population over time and is defined as:

$$y_{imt} = \alpha t_t + \gamma g_m + x'_{imt}\beta + \tau SP_{imt} + \eta_i + \delta_t + \mu_m + \varepsilon_{imt} \tag{2.6}$$

where y_{imt} refers to all health and utilisation outcomes for individual i residing in municipality m at time t; the estimation of the programme effect is the coefficient τ on the interaction of time and eligibility $SP = t * g$; the covariates x_{imt} account for time-varying, municipality, household and individual socio-demographic characteristics; α represents the time-specific group-invariant effects; g_m are the group-specific time-invariant effects; and ε_{imt} is a random error term that captures all omitted factors and is assumed to be uncorrelated with other regressors. Additional individual, η_i, municipal, μ_m, and year, δ_t, fixed effects are included to control for differences across individuals, municipalities and years. Standard errors are adjusted for clustering at the individual level.

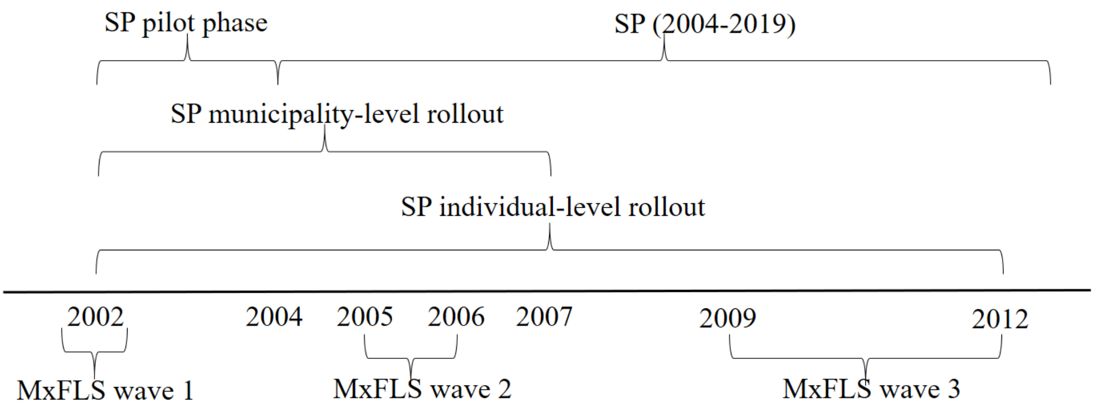

Fig. 2.3 Timing of *Seguro Popular* roll-out and MxFLS sample

A second approach to estimate the programme impacts is a two-way fixed effects (TWFE) model. This specification only considers eligible population. As described earlier, 70% of the municipalities adopt the programme between waves 1 and 2; and the remaining 30% between the second and third waves. All municipalities become treated by the third wave, which means the untreated group is empty for the third wave. The late adopter municipalities are used as a control in this specification. To estimate the intention-to-treat effects, this approach defines a panel data regression model:

$$y_{imt} = x'_{imt}\beta + \tau SP_{imt} + \eta_i + \delta_t + \mu_m + \varepsilon_{imt} \tag{2.7}$$

where the outcomes y_{imt} are regressed on the roll-out indicator SP_{imt} that takes a value of one for municipalities that are participating in *Seguro Popular* in a given year and 0 if they have not implemented the programme yet. As in the specification described in equation 2.6, the time of treatment is re-centred for all municipalities so that all observations before $t = 0$ belong to the pre-treatment period and all observations after treatment belong to the post-treatment period. The approach includes fixed effects for years, δ_t, to capture any time-variant shocks and individual, η_i, and municipal, μ_m, fixed effects to control for time-invariant fixed effects. Standard errors are adjusted for clustering at the individual level and ε_{imt} is a random error term that captures all omitted factors and is assumed to be independent and identically distributed.

The previous two specifications (eqs. 2.6 and 2.7) depend on the validity of the parallel trends assumption, which means that the differences between treated and control populations do not vary over time. To test that assumption, this section presents an event study model as the main strategy to find programme effects. The approach allows for heterogeneous treatment effects depending on the length in time of exposure to treatment before and after the policy expansion. Moreover, it looks for any trends in the outcomes in the pre-treatment periods. The impact of a programme like SP is likely to vary over time as demand for health services increase with affiliation of additional households (Knaul et al., 2012). The treated population is defined as eligible individuals (those without social security coverage) living in municipalities where the programme has already been adopted, and the controls are eligible individuals from municipalities where the programme has not been adopted yet. There is information from six years over three waves (2002, 2005-2006, 2009-2012) and four possible years of adoption (2004, 2005, 2006 and 2007). That means there are individuals observed from 5 years before implementation (those observed in 2002 in municipalities that adopted in 2007) to 10 years after implementation (observed in 2012 that adopted in 2002)(see Fig. 2.3).

Defining years under treatment j as the difference between year of observation and year of adoption, equation 2.8 shows treatment effects represented by the coefficients τ_j of the interaction between year of observation t and year of implementation T:

$$y_{mit} = \eta_i + \mu_m + \delta_t + x'_{mit}\beta + \sum_{j=-5}^{-2} \tau_j^0 SP_{mt} + \sum_{j=0}^{10} \tau_j^1 SP_{mt} + \varepsilon_{imt} \tag{2.8}$$

where SP is an indicator that equals one if the municipality m offers the programme in time t; the τ_j terms represent the differential effect for each year separately from 5 years prior to the programme to 10 years after. The dummy variable that corresponds to the period of one year before treatment $j = -1$ is omitted and taken as the base year similarly to Conti and Ginja (2020) and Bailey and Goodman-Bacon (2015). The first set τ^0 captures all the differences if any by year before the programme, and the second set τ^1 are the differential effects by year under treatment, all effects are estimated with respect to $j = -1$. So that any statistical significance in the pre-treatment τ coefficients would imply a violation of the parallel trends assumption. The fixed effects terms of equation 2.8 are η_i, to control for time-invariant individual unobservables; μ_m, a set of dummy variables to account for municipality-level effects; δ_t is the effect on any period common to all units; covariates x_{imt} account for time-varying household and individual socio demographic characteristics; and the error term ε_{imt}, captures all the remaining omitted factors and is assumed to be random and independent from other regressors.

The models described so far account for variations in time and space in the roll-out, but not for programme intensity – understood as the proportion of people affiliated each year with respect to the maximum number of people reached in a single year over the programme's life (2020-2019) –. Those models assume that the programme has been rolled-out in a given municipality if at least 10 households are beneficiaries of the programme consistent with previous studies (Bosch and Campos-Vazquez, 2014; Conti and Ginja, 2020; Del Valle, 2014). This assumption does not take into account differences in the proportion of people that are in fact enrolled in the programme. The following specification is a model that includes an indicator of coverage at the population level to a triple-differences specification that takes differences in intensity of coverage by municipality over time (pre and post-treatment) and individual SP eligibility (covered by social security or not). The model is similar to the one used in Barros et al. (2008) except for the definition of intensity, in their case with respect to the target population and in this case with respect to population eventually enrolled. The definitions of groups is similar to the specification in equation 2.6, where the treated are individuals with no social security affiliation after roll-out and the controls are those with social security benefits. The model estimated is:

$$\begin{aligned} y_{imt} =& \tau_1 int_{mt} + \tau_2 g_i + \tau_3 t_t + \tau_4 int_{mt} g_i + \tau_5 int_{mt} t_t + \\ & \tau_6 g_i t_t + \tau_7 int_m g_i t_t + x'_{imt}\beta + \eta_i + \mu_m + \delta_t + \varepsilon_{imt} \end{aligned} \tag{2.9}$$

where the coefficient of β_7 measures the programme effects as the triple interaction of municipal intensity, time and eligibility. Programme intensity int_{mt} is calculated as the ratio of number of people covered by SP in a municipality on a given year divided by the maximum number of people covered in any year between 2002 and 2019. Intensity in this context can be interpreted as the probability of an individual i in a treated municipality m at year t to be affiliated to the programme. The model includes the three variables, the three double interactions and a set of individual covariates. Programme intensity can be interpreted as the probability that a given individual is covered by the programme in a given year. Therefore, it tries to proxy actual treatment effects by multiplying the intention-to-treat effects by the probability of being covered by the programme. The model includes individual, η_i, municipality, μ_m, and year, δ_t, fixed effects to account for the arbitrary roll-out across municipalities; and ε_{imt} is the idiosyncratic error assumed to be random, independent and identically distributed.

2.6.2 Average treatment effects

The previous section describes the main strategy of correcting the possible selection bias by estimating intention-to-treatment effects. In that case, the programme impact is estimated on the eligible population but not necessarily on the population effectively treated. This section presents an alternative strategy in which the treated population are those families that reported being affiliated to *Seguro Popular*; and the controls are those individuals who are eligible to receive the programme, i.e. are not covered by the social security, but are not enrolled in *Seguro Popular*. As affiliation the programme is voluntary, there is a risk that the endogenous decision to enrol introduce a bias in the estimations. The possible selection bias is addressed by the combination of matching and difference-in-differences approaches. This specification controls for observable and unobservable sources of bias by matching affiliates to non-affiliates at the baseline level using entropy balance; and by estimating the programme impact using difference-in-differences design. Programme beneficiaries were matched to eligible population but not SP recipient using socio-demographic characteristics such as age, sex, years of education, marital status, household income, and whether the household is in a rural area. The sample was preprocessed by entropy balancing to reach covariate balance between the beneficiary population and the non-beneficiaries. The approach consists of a maximum entropy re-weighting scheme that generates the weights that balance the treated and control groups with respect to a set of covariates (Hainmueller and Xu, 2013). Entropy balance provides a generalisation of the propensity score approach without the need for computing the balance checks to verify that the weights match the covariate distributions. The method allows for balance with respect to the mean or to higher moments (variance, skewness) (Hainmueller, 2012). The resulting weights are used to estimate the treatment effects using a weighted difference-in-differences model. To ensure that treated individuals have comparable observations with the control group, only those observations within the common support are used in the estimations.

Table 2.2 Matrix of populations and periods for ATT of *Seguro Popular*

	Period	
Group	Non-affiliated pre treatment	Non-affiliated post treatment
	Affiliated pre treatment	Affiliated post treatment

Notes. This table shows population and timing definitions for equation 2.10.

There are two different specifications using the actual treatment indicator. The survey only includes information about *Seguro Popular* affiliation for the second (MxFLS-2) and third (MxFLS-3) waves. Therefore, the first specification compares outcomes from affiliates to non-affiliates from those waves using a treated only population who shift from untreated to treated (see Table 2.2). The second approach includes the first wave into the analysis assuming no one was treated in 2002. The assumption is reasonable as the programme was implemented in 2002 and it is unlikely that by the time of the survey in the same year, any treated population did experience already an impact. Moreover, only 7% reported being enrolled by the second wave, therefore, the percentage insured by the first wave was probably significantly lower. The model defined to estimate average treatment effects on the treated (ATT) for both specifications is defined as:

$$y_{it} = \alpha t_t + \gamma g_i + \tau SP_{it} + x'_{it}\beta + \eta_i + \mu_m + \delta_t + \varepsilon_{it} \tag{2.10}$$

where y_{it} are health and utilisation outcomes at the individual level; the coefficient τ of the interaction between group and time represents the estimation of the programme effect; the covariates x_{it} account for time-varying household and individual socio-demographic characteristics; α_t are the time-specific group-invariant effects; γ_i are the group-specific time invariant-effects; η_i individual fixed-effects and ε_{it} is a random error term that captures all omitted factors.

In summary, there are six strategies to test the hypothesis that SP improved health status; the first is an intention-to-treat effects estimation that compares outcomes from eligible to non-eligible population before and after municipality-level roll-out; the second is a two-way fixed-effects estimation on eligible population before and after treatment; the third approach is a felixible event study design that captures the effect in time and tests the parallel trends assumption; the fourth is a model of triple differences that takes into account programme's intensity; the fifth strategy consisted in comparing the population that reported being affiliated to the programme with eligible population that reported non-affiliation to *Seguro Popular* using two MxFLS: and finally a sixth strategy extends it to include three waves of data. The primary approach is the event study design with the other five are used to substantiate those results.

2.7 Results

2.7.1 Intention-to-treat estimations

Descriptive statistics.

Table 2.3 shows pre- and post-treatment summary statistics of the complete set of outcomes and control variables for eligible population (people without social security) and non-eligible population (people with social security). The socio-demographic characteristics used as covariates at the individual level were age, sex, civil status and years of education; at the household level the covariates include household size, asset index, rural area and whether the household receives *Oportunidades*. On average, as the survey follows the same individuals over time, age after treatment (43) is higher to before treatment age (39); before treatment the sample was 44% male, and 42% after treatment; the percentage of married population is 64%, slightly lower before treatment, compared to 69% after treatment; years of education increase slightly after roll-out, from 6.3 to 6.51. On household level characteristics, household size is larger before roll-out, 6.32 members on average compared to 6.06 before roll-out; asset index is 5.07 before rollout and 5.51 after roll-out; the percentage of treated population living in rural areas is higher 48% compared to 43%; and population receiving *Oportunidades* is 23% before treatment and 20% after-treatment; people who exercise regularly dropped from 13% to 11%; finally, people reported exercising more regularly before roll-out and smoking did not change. The next set of variables are health outcomes. Average body mass index (BMI) is higher after roll-out, 26.46 compared to 27.76; the percentage of overweight is 35% before and 37% after roll-out; prevalence of obesity increased from 22% to 30%; blood pressure level was higher at the pre-programme period, it was 9% compared to 7% before the programme introduction; anaemia levels were higher at baseline, 14% against 10% at follow-up. On the self-reported health side, the proportion of people that consider their health to be good remained constant at 46%; but those who thought health was better than last year declined from 22% at pre-treatment to 20% after the policy expansion; similarly, the proportion of people believing their health will be better next year dropped from 33% before roll-out to 30% after roll-out; the percentage of people reporting having good health considering age and gender had a slight increase from 28% to 29% in average. For healthcare utilisation, the proportion of people who had an outpatient visit to a hospital or clinic remained constant at 11% and outpatient visits to doctor, only 1% both before and after roll-out; hospitalisations increased; from 4% to 5%; and the likelihood of having health insurance had a striking increase from 2% to 26% as people enrolled to SP.

Health outcomes

Table 2.4 panel 1 displays programme intention-to-treat effects of SP, before and after the municipality of residence implemented the programme. The effects on population's health levels are estimated by the approach described by equation 2.6, where the model takes the differences on eligibility (not having social security protection) over time. The term that captures the programme effects is the interaction between the indicator of eligible individuals and the post-treatment indicator. Columns 1 and 2 show results on hypertension for the full sample and for the poorer quintile of the sample respectively. Columns 3 and 4 refer to ITT effects on anaemia for both samples. There are no significant effects in any of the two outcomes for the full sample or the poorer population sample. All models include individual, municipality and year effects.

The second panel in Table 2.4 shows the results of the two-way fixed effects design described in equation 2.7 where the treatment variable is an indicator of a municipality already participating in the programme with individual, municipality and time fixed effects. The signs of the coefficients have the expected direction for both outcomes and both samples but are statistically insignificant.

Panel 3 of Table 2.4 shows the programme impacts of the triple-difference model described in equation 2.9. The model takes differences in programme municipality intensity over time (pre and post-treatment) and individual SP eligibility (covered by social security or not). Programme intensity is defined as the proportion of individuals affiliated to SP in a municipality in a given year with respect to the maximum affiliation reached by that municipality in any year between 2002 and 2019. This can be interpreted as the probability of being actually affiliated, therefore, an approximation to average treatment effects rather than intention-to-treat-effects. The likelihood of having hypertension or anaemia when changing the intensity of programme affiliation did not seem to be affected.

Finally, the main results on health outcomes are shown on panel 4 of Table 2.4. Treatment effects are estimated by breaking up the programme's impacts on health by time of exposure in a municipality as described in equation 2.8. In this approach, there are three observations for each individual (2002, 2005-2006, or 2009-2012) located in municipalities where the programme was implemented in any of the five years between 2002 and 2007. Therefore, the survey has individuals from five years before roll-out (observed in 2002 and roll-out in 2007), to 10 years after roll-out occurred in a municipality (observed in 2012, roll-out in 2002). Time of exposure is presented by year from -5 to 7 or more years of exposure. No effects of the introduction of *Seguro Popular* over time are found with this specification for the full sample or when considering only the poorer quintile in the sample.

Self-reported health status

Table 2.5 shows the programme's effects on some self-reported health indicators. In panel 1, the difference-in-differences estimator finds no effects on self-reported health; similarly to panel 2, the TWFE estimates. In panel 3, when accounting for the geographical intensity of the programme, results show that people in the poorer quintile subsample reported having 2.13 percent points worse health for every additional percent point in programme coverage. This unexpected effect could be caused by intense individual enrolment in areas with with greater need in terms of healthcare access; and poorer population. The effect can be explained by health status underlying levels in those specific areas. Only after the programme is well established on those areas, we may expect an effect on health levels, and in self-reported health status. Taking into account differences in time of exposure to the programme, defined in equation 2.8, panel 4 shows no statistically significant effects on the self-perception of health derived from different years after the introduction of *Seguro Popular*.

Healthcare utilisation

Tables 2.6 and 2.7 show intention-to-treat effects of SP on healthcare utilisation indicators. In panel 1, Table 2.7 as expected, there is a positive effect on eligible population (no social security protected living in a municipality that adopted the programme) of 24.8 percentage points on the likelihood of having public or private health insurance and 29.6 percentage points on poorer population. There is no effect in outpatient visits in the last four weeks or hospitalisations in the previous year.

In panel 2, Table 2.7 the effects of roll-out over time had no effect on the likelihood of having health insurance. Moreover, Table 2.6 shows no impacts of the programme roll-out on outpatient visits or hospitalisations.

When taking into account the geographical intensity, defined as the proportion of affiliates in a municipality in a given year with respect to the maximum number of affiliates in that municipality in any year, Table 2.6 shows that SP had no effect on healthcare visits. Table 2.7 shows a positive effect of SP on health insurance uptake of 1.62 p.p. with an increase in intensity of 1 p.p. and an increase of 1.23 p.p. in health insurance uptake for every p.p. of expanded coverage. The positive effects on health insurance uptake with an increase in coverage are to be expected.

Panel 4 in Table 2.6 shows no effect on healthcare visits. Table 2.7 on a event-study design, shows the effect of SP across time. Time since SP introduction has a positive effect on health insurance uptake. Moreover, health insurance uptake increases the longer *Seguro Popular* has been operating in a municipality for both samples. The effect for poorer population is larger, which is consistent with *Seguro*

Popular's target population. These main findings estimated by equation 2.8 are also shown in Figures 2.4 and 2.5, where the coefficients of intention-to-treat-effects seem to be no different than zero for all outcomes except for health insurance uptake.

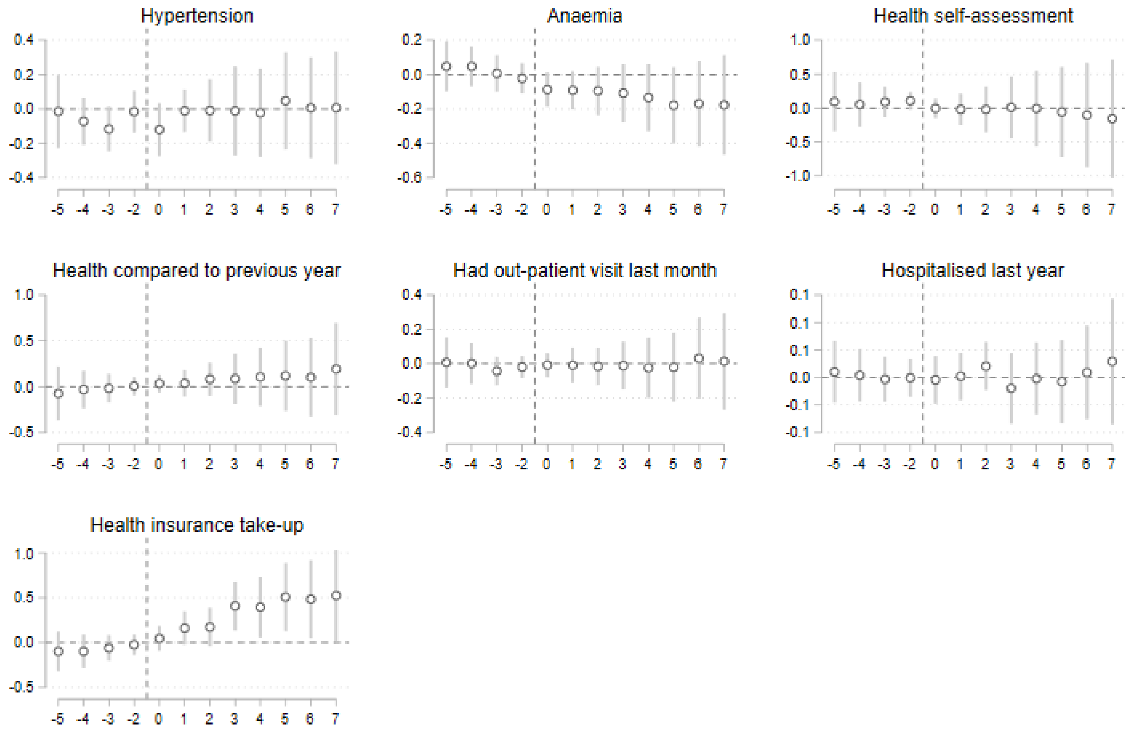

Fig. 2.4 Intention-to-treat effects of *Seguro Popular*

Mortality

This section describes intention-to-treat effects of *Seguro Popular* on mortality rates by age group shown in Figures 2.6 and 2.7. To construct mortality rates, deaths data was aggregated at the municipality-year level for years 2000 to 2017. Programme impacts were estimated by an event-study design specification described in equation 2.8. Regressions were weighted by the population size of each age group. Time of exposure was divided in years from six years or more years before roll-out to 14 years or more after roll-out. The year prior to implementation is used as the reference year as other studies (Conti and Ginja, 2020; Miller et al., 2021). This specification has the advantage of including past periods for any existing trends to verify that the parallel trends assumption is satisfied.

Table 2.13 shows the effect of SP on infant mortality rates by municipality for all and for specifically poorer municipalities. Results show a clear reduction in the infant mortality rate from 2.2 deaths per 1000 births on poor municipalities after one year in the programme, to over 24 deaths after roll-out. Given that the weighted

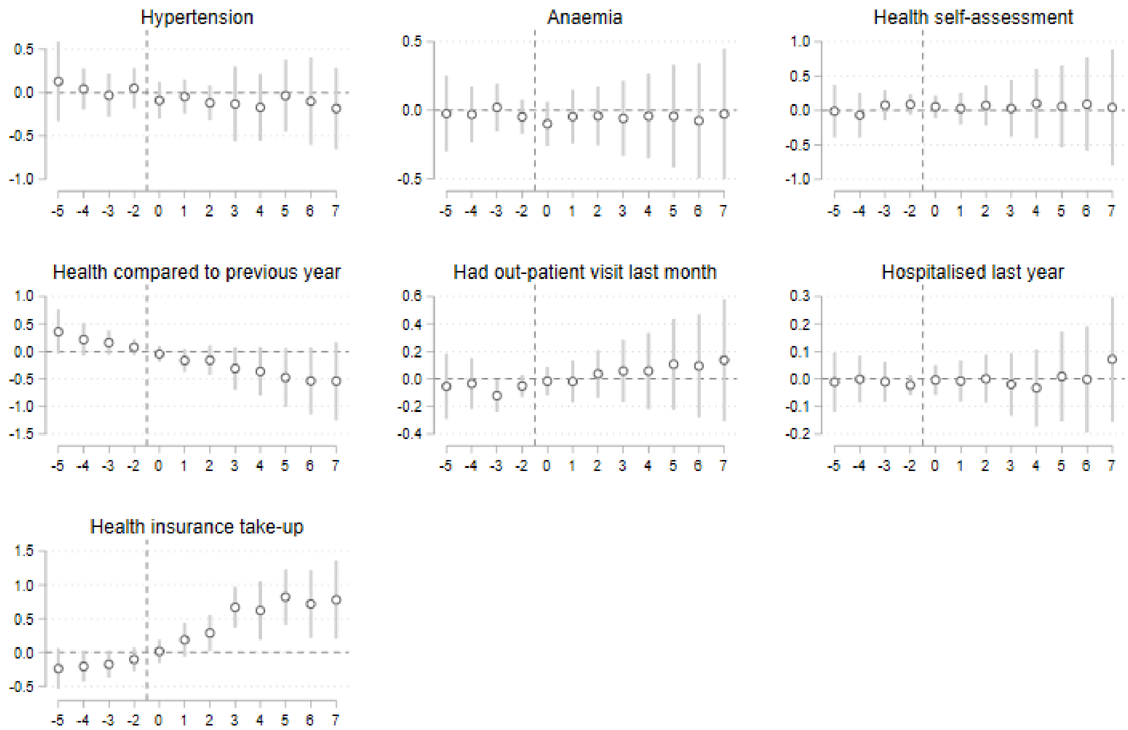

Fig. 2.5 Intention-to-treat effects of *Seguro Popular* on poor population

average infant mortality rate was estimated at roughly 13 deaths per 1000 births in poorer municipalities, it is an important decline as shown in Figure 2.7. When including all municipalities there seems to be a mild mortality rate increasing trend before and after the introduction of SP but it is not statistically significant. For children under 10 years of age, Table 2.14 shows no effect on the full sample, but a decline in mortality due to the introduction of SP on poorer municipalities. However, as shown in Figure 2.7, mortality rates seem to be following a trend from before the introduction of *Seguro Popular* which would suggest a violation of the parallel trends assumption. Among adults over 20 years of age and over 60 years of age, there are not intention-to-treat impacts of SP on weighted mortality rates for the whole population or for the sample restricted to marginalised and underdeveloped areas.

2.7.2 Average treatment estimations

Descriptive statistics

Table 2.8 shows summary statistics for the baseline sample before and after matching for beneficiaries of SP and non-beneficiaries. The entropy balancing routine was implemented for the first, second and third statistical moments of the outcomes and covariate distributions (variances and skewness are not shown here). Table 2.8 shows

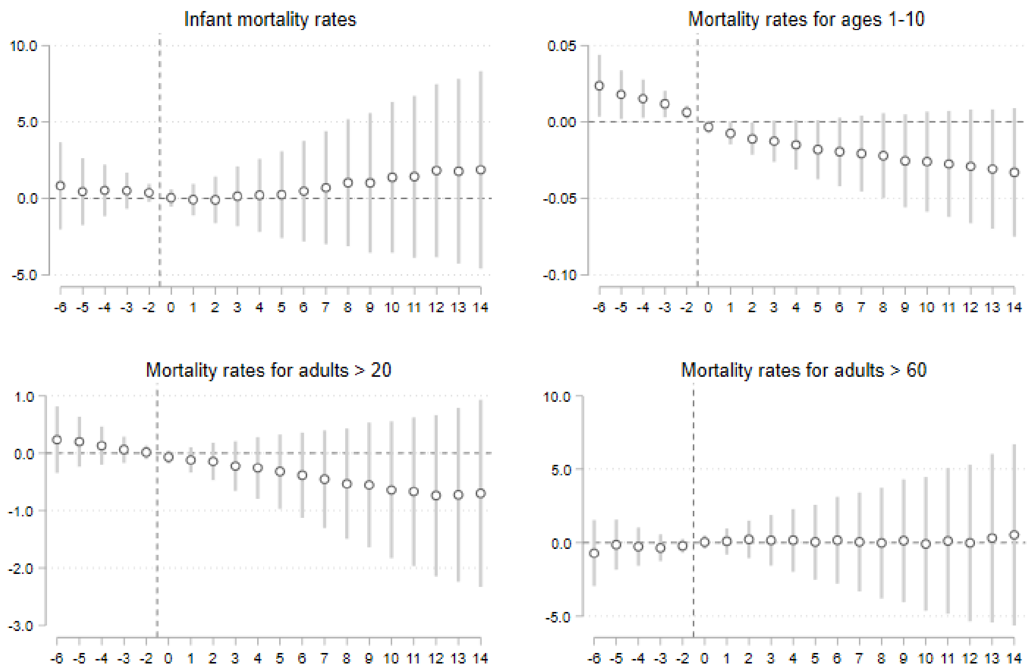

Fig. 2.6 Intention-to-treat effects on mortality rates

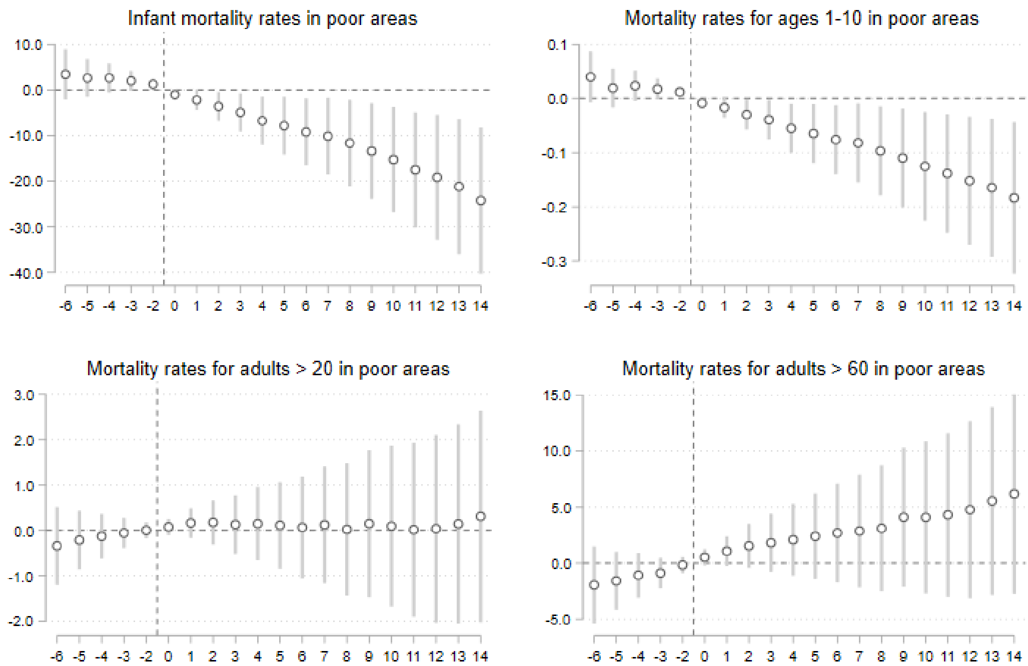

Fig. 2.7 Intention-to-treat effects on mortality rates in poor areas

that, after the matching routine, the means of non-beneficiaries and beneficiaries are similar. The assumption is that once the samples are balanced, any difference between the outcomes of the treated and the comparisons will be due to membership to *Seguro Popular*. There is one year difference between affiliated and non-affiliated, this difference remains after balancing the samples. Sex of recipients was 43% male and 46% among non-recipients; 59% of affiliates were married, while for non-affiliates the share was 51%; recipients had 6.92 years of education, while non-recipients had 7.68 in average; household size was 7.41 among recipients while non-recipient households had in average 6.21 members; recipients were poorer; and more likely to be a Oportunidades beneficiary, 21% compared to 8% among SP recipient. SP enrolees had higher BMI and more likely to be overweight or obese. Among non-recipients, 36% were overweight and 27% were obese, for recipients, the proportions were 38% and 28% respectively. Prevalence of anaemia was 13% and hypertension 8% for both groups. Only 49% of beneficiaries reported good health, in comparison to 58% of non-beneficiaries; recipients were 9% likely to leave activities for health reasons, compared to 8% among non-recipients. Recipients were also more pessimistic about their health, 23% reported health being better than the previous year, compared to 26% of non-recipients; 34% predicts their health will be better the following year, compared to 38% for non-SP covered; and 30% considers their health is good compared to someone of same sex and age; for non-recipients the share was 34%. For health utilisation, 13% of *Seguro Popular* affiliates had an outpatient visit to a hospital or clinic in the previous 4 weeks, compared to only 10% of non-affiliates; 1% of both groups had an outpatient visit to the doctor; 5% of beneficiaries were hospitalised in the last 12 months, for 4% of non-beneficiaries; among SP recipients the 32% had health insurance, while only 7% among non-recipients. Given that the definition of recipient is at least one person in the household enrolled to *Seguro Popular*, it is possible that, individually, only 32% are covered by health insurance.

Health outcomes

Table 2.9 shows average treatment effects on the treated using difference-in-differences estimation on health outcomes. Taking the difference of individual affiliation in time, as described in equation 2.10, using two MxFLS waves without matching, first panel results suggest no apparent effects of the policy on the complete sample or on the lowest quintile of income sample. The second panel, combining difference-in-differences with entropy balance on two waves, show a negative effect of the programme, however those are not statistically significant for either sample. The third panel, using two-way fixed effects regressions, shows no effect of affiliation on the three-wave unmatched sample in average or for the lowest income quintile. The fourth panel, shows no effects on the balanced three-wave panel in general or for the poorer population sample.

Self-reported health status

Table 2.10 displays the effects on self-reported health status estimated as described bt equation 2.10. Panel 1 and 2, show no effects on the unbalanced or in the balanced difference-in-difference estimations for the full sample and when restricting to the poorer quintile. Panel 3, when adding the third wave in a TWFE design with an unmatched sample, shows no effects. However, for the balanced sample in panel 4, there is a small negative effect of 5 percent points on health self-assessment of affiliating to SP for the entire sample, however, it is only marginally significant ($p<0.1$).

Healthcare utilisation

The difference-in-differences estimation using two MxFLS waves, in Table 2.11, shows no change in outpatient visits related to SP affiliation for the full sample (columns 1 and 3) or for the lowest income quintile (columns 2 and 4) for both matched and unmatched samples (panels 1 and 2). For the matched two-way fixed effects specification in panel 4, results show a positive effect of healthcare utilisation resulted from affiliating to SP on the full sample. Outpatient visits had 2.9 percentage points increase and hospitalisations had a rise of 1.6 p.p. on average. Table 2.12 as expected shows a strong and positive effect of *Seguro Popular* on the likelihood of having health insurance for all specifications and both populations. Average treatment effects on the treated of affiliating to SP varied from 65 to 75 p.p. in the likelihood of taking-up health insurance.

2.8 Discussion

The implementation of the *Seguro Popular* in Mexico had the objective to close the gap in access to healthcare and health status between the insured and uninsured population by reducing the financial barrier to services. The differences in access between those two groups where the uninsured had to pay for healthcare worsened the existing inequalities. While existing literature suggests that SP has been successful as a financial protection mechanism (Barros et al., 2008; King et al., 2009; Sosa-Rubí et al., 2011), the evidence on whether or not health outcomes are improved is less definitive. The present study focused on determining the programme's effect on health status and healthcare utilisation.

This study used a few model specifications with different identification strategies. First, it estimated intention-to-treat effects of eligible population compared to non-eligible; the second, using only eligible population estimated a two-way fixed effects model; the third is a triple difference specification that extends the first by adding a variable that captures the probability for an individual of being treated, where the treatment effects are the triple interaction of probability of being treated by eligibility over time; the fourth intent-to-treat estimate, the main strategy, analyses the effects by the number of years on the programme and the controls are the same individuals before treated. Finally, using the actual treatment indicator, a difference-in-difference approach using two waves and a TWFE on the three waves was run on a previously matched sample.

Results of these analyses show an effect on health status. Even though there is no apparent impact on morbidity measured by hypertension or anaemia, there is an effect found in reducing mortality rates of children in poorer areas. Infant mortality rates had a clear impact; while mortality rates of older children had a modest impact of less than one death by 1000 children; impact on mortality rates of adults had no effect. One explanation for a lack of impact on morbidity is inadequate data. The MxFLS panel dataset used for assessing SP effect on health outcomes and utilisation has data up until 2010[9], while the administrative dataset use for assessing mortality rates has data for 2017. According to results in table 2.13, there is an impact of the programme on infant mortality rates right after SP is introduced. However, the effect gets statistically stronger 10 years or more after the programme has been introduced in a municipality. It is possible that impact on different health outcomes will also manifest in the long run. Estimated impact on healthcare coverage is consistent with the long run effect of *Seguro Popular* on health outcomes. According to results, the effect of the programme on coverage gets greater with time under treatment, and stabilises as shown in Figs. 2.4 and 2.5; or when adding a third wave as shown in table 2.12.

SP effect on perception of own health for poorer population seems to show a negative impact for some specifications. Evidence suggests that even in the context of universal coverage, poorer population living in marginalised areas have less access to healthcare in comparison with higher income households (Bautista-Arredondo et al., 2014; Colchero et al., 2019). The challenge is to reduce inequalities in access. There is a growing literature on the effects of the universalisation of healthcare on financial protection, increasing access and improving the health status of the population (Acharya et al., 2013; Erlangga et al., 2019). However, the historical

[9]MxFLS-3 provides some data on 2011, 2012 and 2013 but only about 5% of the full three-wave dataset.

structure of the Mexican health system where access to higher quality healthcare is linked to labour status introduced deep inequalities. Wagstaff (2010) finds that this type of system with payroll financing fares badly covering the non-poor informal sector until the economy has reached a high level of economic development. In Mexico, the prevailing system has underperformed providing health coverage to also the poor informal sector. While there are examples in the world where more than one protection schemes can coexist, the pooled mechanism for the uninsured without the full pack of benefits of social security has not been the solution as it does not address the problem of inequality.

This analysis has some limitations. The dataset used for the morbidity and utilisation outcomes spans from the start of the programme in 2002 to the end of the individual level enrolment. However, using longer time series would help provide a better assessment of the programme. Results from this analysis and other studies suggest that the effects are probably larger on the long run, about 8 to 10 years after implementation. Additional data points after 2012 would shed some light on potential long-run effects. Likewise, the fact that the survey does not include information from before 2002, makes it challenging to examine pre-existing trends. Having additional data points before implementation would have been useful to disentangle the effects of *Seguro Popular*. The limited number of health outcomes in the survey is another challenge of this study. A few other health variables were added in the third round and those could be analysed using a different approach or using a similar one if there is a fourth round. Another limitation related to the available data is the lack of information on *Seguro Popular* uptake in the first round. This is probably not a serious limitation as the proportion of people that already had the programme in 2002 was very low.

There is a limitation related to the presence of endogeneity in the voluntary programme affiliation. The resulting bias is reduced by the approaches used as identification strategies but it is possible that some bias remains in the estimates. The municipality level identification might reduce the self-selection bias, but as the programme was not randomly assigned at the municipality level, some bias may persist. Including municipality fixed effects in the regressions helped mitigate against such biases even further. Similarly, combining difference-in-differences approach with entropy balancing on the individual-level analysis, helped reducing bias. Although the remaining level of bias is a limitation, the multiple specifications used may convince the reader it is not concerning.

One of the contributions of this study is the choice of metabolic conditions as outcomes. The effect of *Seguro Popular* has been largely evaluated on mortality and childhood and reproductive conditions but to a much lesser degree on metabolic health outcomes. Sosa-Rubí et al. (2009) find an increase in access to healthcare services and a health improvement on diabetic patients. The present study does not find an improvement on metabolic health of affiliates. A contribution is the use of long time series data on mortality covering almost the totality of the programme duration since before its start. Similar analyses on mortality rates covers a shorter period (Conti and Ginja, 2017). This analysis builds up on previous work and finds insight from long-run exposure. This type of analysis may contribute to understand the long-run effects of the programme.

Table 2.3 Descriptive statistics

Variable	Before roll-out N	Mean	Median	After roll-out N	Mean	Median
Socio-demographic						
Age	9,685	38.81	36	10,753	43.63	41
Sex (male=1)	9,812	0.44	0	11,563	0.42	0
Civil status (married=1)	10,072	0.64	1	11,564	0.69	1
Years of education	7,967	6.31	6	8,975	6.51	6
Household size	10,072	6.32	6	11,565	6.06	5
Asset index	9,319	5.07	5.34	11,436	5.51	5.83
Rural area	10,072	0.43	0	11,565	0.48	0
Oportunidades recipient	8,616	0.23	0	8,839	0.2	0
Ever had serious accident?	10,072	0.1	0	11,563	0.1	0
Over 50 years old	10,072	0.26	0	11,564	0.33	0
Exercises regularly	10,072	0.13	0	11,564	0.11	0
Has ever smoked	10,072	0.14	0	11,564	0.14	0
Health outcomes						
Body mass index	9,282	26.46	25.83	10,313	27.76	27.25
Overweight	9,282	0.35	0	10,313	0.37	0
Obesity	9,282	0.22	0	10,313	0.3	0
Hypertension	8,779	0.09	0	10,737	0.07	0
Anaemia	8,267	0.14	0	8,581	0.1	0
Self-reported health						
Reports good health	10,072	0.46	0	11,564	0.46	0
Left activities for health related reasons last 4 weeks	10,072	0.09	0	11,563	0.1	0
Reports health to be better than last year	10,071	0.22	0	11,552	0.2	0
Predicts health to be better next year	10,070	0.33	0	11,562	0.3	0
Reports good health for age/gender	10,071	0.28	0	11,562	0.29	0
Healthcare utilisation						
OP visit to hospital/clinic during last 4 weeks	10,068	0.11	0	11,532	0.11	0
OP visit to dr/healer during last 4 weeks	10,068	0.01	0	11,531	0.01	0
IP visit to hosp/clinic during last 12 months	10,067	0.04	0	11,533	0.05	0
Social security/private health insurance	10,072	0.02	0	11,565	0.26	0

Notes. This table shows a descriptive summary of the sample contained in MxFLS1-3. Before roll-out refers to the data from municipalities where *Seguro Popular* was not yet rolled-out. After roll-out refers to those municipalities that were already affiliated to *Seguro Popular*.

Table 2.4 *Seguro Popular* intention-to-treat effects on health outcomes

	(1) Hypertension all	(2) Hypertension poor	(3) Anaemia all	(4) Anaemia poor
1. Difference-in-differences	0.00946	-0.00531	0.00169	-0.0137
	(0.0148)	(0.0332)	(0.0186)	(0.0497)
Observations	16,167	5,129	12,332	3,596
2. Two-way fixed effects	-0.0222	-0.0313	-0.0347	-0.0549
	(0.0195)	(0.0294)	(0.0325)	(0.0526)
Observations	8,521	3,615	6,215	2,453
3. Triple differences	0.0288	0.659	-0.00871	0.0316
	(0.0218)	(0.418)	(0.0357)	(0.688)
Observations	16,167	5,129	12,332	3,596
4. Event-study model				
year -5	-0.115	-0.0439	0.0476	-0.0245
	(0.0728)	(0.131)	(0.0729)	(0.140)
year -4	-0.116**	-0.0657	0.0472	-0.0312
	(0.0503)	(0.0863)	(0.0584)	(0.102)
year -3	-0.0840*	-0.00403	0.00653	0.0208
	(0.0438)	(0.0703)	(0.0534)	(0.0874)
year -2	-0.0297	0.0215	-0.0215	-0.0476
	(0.0358)	(0.0554)	(0.0441)	(0.0628)
year 0	-0.0127	0.00115	-0.0873*	-0.0990
	(0.0314)	(0.0467)	(0.0498)	(0.0810)
year 1	0.0119	0.0130	-0.0914	-0.0461
	(0.0400)	(0.0652)	(0.0560)	(0.0998)
year 2	0.0369	0.0542	-0.0950	-0.0409
	(0.0458)	(0.0701)	(0.0713)	(0.108)
year 3	0.0843	0.0389	-0.108	-0.0588
	(0.0567)	(0.117)	(0.0848)	(0.138)
year 4	0.0639	0.0406	-0.134	-0.0425
	(0.0707)	(0.129)	(0.0982)	(0.155)
year 5	0.0934	0.101	-0.179	-0.0438
	(0.0812)	(0.147)	(0.113)	(0.188)
year 6	0.104	0.0740	-0.170	-0.0763
	(0.0937)	(0.178)	(0.125)	(0.211)
year 7	0.105	0.0616	-0.176	-0.0276
	(0.101)	(0.162)	(0.146)	(0.239)
Observations	8,521	3,615	6,215	2,453

Notes. Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1.
This table displays intention-to-treat effects of *Seguro Popular* on health outcomes. Panels 1 & 2 are estimations resulted from using equation 2.6; panel 3, uses equation 2.9 and panel 4 are estimations from equation 2.8. All estimations include year and municipality fixed effects and a set of socio-demographic covariates including age, sex, civil status, years of education, household size, asset index, rural residence and membership to *Oportunidades*.

Table 2.5 *Seguro Popular* intention-to-treat effects on self-reported health

	(1) Health self-assessment all	(2) Health self-assessment poor	(3) Health compared to last year all	(4) Health compared to last year poor
1. Difference-in-differences	0.0225	-0.00997	-0.00193	0.00955
	(0.0238)	(0.0213)	(0.0530)	(0.0451)
Observations	16,695	16,682	5,299	5,295
2. Two-way fixed effects	-0.0276	0.00653	-0.0472	0.0341
	(0.0433)	(0.0315)	(0.0598)	(0.0364)
Observations	8,805	8,796	3,734	3,731
3. Triple differences	-0.625	-2.135***	-0.109	-0.730
	(0.473)	(0.672)	(0.565)	(0.952)
Observations	16,695	16,682	5,299	5,295
4. Event-study model				
year -5	0.0948	-0.0755	-0.00861	0.358*
	(0.220)	(0.147)	(0.193)	(0.203)
year -4	0.0539	-0.0308	-0.0680	0.217
	(0.166)	(0.103)	(0.164)	(0.150)
year -3	0.0897	-0.0169	0.0775	0.159
	(0.113)	(0.0782)	(0.112)	(0.112)
year -2	0.107	0.00550	0.0889	0.0771
	(0.0665)	(0.0508)	(0.0746)	(0.0732)
year 0	-0.00412	0.0338	0.0530	-0.0448
	(0.0732)	(0.0484)	(0.0824)	(0.0718)
year 1	-0.0191	0.0370	0.0288	-0.166
	(0.117)	(0.0733)	(0.117)	(0.106)
year 2	-0.0200	0.0814	0.0737	-0.158
	(0.170)	(0.0912)	(0.148)	(0.135)
year 3	0.0109	0.0862	0.0293	-0.311
	(0.230)	(0.138)	(0.208)	(0.194)
year 4	-0.00709	0.105	0.101	-0.364
	(0.281)	(0.161)	(0.254)	(0.222)
year 5	-0.0571	0.118	0.0583	-0.474*
	(0.336)	(0.193)	(0.298)	(0.268)
year 6	-0.104	0.103	0.0911	-0.537*
	(0.389)	(0.216)	(0.342)	(0.307)
year 7	-0.155	0.192	0.0411	-0.540
	(0.441)	(0.254)	(0.424)	(0.359)
Observations	8,805	8,796	3,734	3,731

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes. This table presents intention-to-treat effects of *Seguro Popular* on self-reporting health status. Panels 1 & 2 are estimations resulted from using equation 2.6; panel 3, uses equation 2.9 and panel 4 are estimations from equation 2.8. All estimations include year and municipality fixed effects and a set of socio-demographic covariates including age, sex, civil status, years of education, household size, asset index, rural residence and membership to *Oportunidades*.

Table 2.6 *Seguro Popular* intention-to-treat effects on healthcare utilisation

	(1) Outpatient visit in last 4 weeks all	(2) Outpatient visit in last 4 weeks poor	(3) Hospitalisation last year all	(4) Hospitalisation last year poor
1. Difference-in-differences	0.00761	0.0347	0.00416	-0.00890
	(0.0180)	(0.0295)	(0.0101)	(0.0190)
Observations	16,668	5,293	16,667	5,292
2. Two-way fixed effects	0.00755	0.0341	0.00731	0.0168
	(0.0210)	(0.0364)	(0.0134)	(0.0291)
Observations	8,790	3,731	8,791	3,730
3. Triple differences	0.0438	-0.730	0.493	0.262
	(0.269)	(0.952)	(0.353)	(0.393)
Observations	16,668	5,295	16,667	5,293
4. Event-study model				
year -5	0.00700	-0.00861	0.0101	0.358*
	(0.0740)	(0.193)	(0.0282)	(0.203)
year -4	0.00195	-0.0680	0.00401	0.217
	(0.0601)	(0.164)	(0.0240)	(0.150)
year -3	-0.0426	0.0775	-0.00329	0.159
	(0.0414)	(0.112)	(0.0206)	(0.112)
year -2	-0.0192	0.0889	-0.000808	0.0771
	(0.0325)	(0.0746)	(0.0175)	(0.0732)
year 0	-0.00765	0.0530	-0.00423	-0.0448
	(0.0354)	(0.0824)	(0.0219)	(0.0718)
year 1	-0.00853	0.0288	0.00211	-0.166
	(0.0516)	(0.117)	(0.0219)	(0.106)
year 2	-0.0153	0.0737	0.0205	-0.158
	(0.0547)	(0.148)	(0.0221)	(0.135)
year 3	-0.0110	0.0293	-0.0194	-0.311
	(0.0702)	(0.208)	(0.0325)	(0.194)
year 4	-0.0247	0.101	-0.00219	-0.364
	(0.0888)	(0.254)	(0.0331)	(0.222)
year 5	-0.0208	0.0583	-0.00741	-0.474*
	(0.101)	(0.298)	(0.0382)	(0.268)
year 6	0.0321	0.0911	0.00896	-0.537*
	(0.119)	(0.342)	(0.0429)	(0.307)
year 7	0.0138	0.0411	0.0291	-0.540
	(0.142)	(0.424)	(0.0577)	(0.359)
Observations	8,790	3,734	8,791	3,731

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes. This table shows intention-to-treat effects of *Seguro Popular* on healthcare utilisation. Panels 1 & 2 are estimations resulted from using equation 2.6; panel 3, uses equation 2.9 and panel 4 are estimations from equation 2.8. All estimations include year and municipality fixed effects and a set of socio-demographic covariates including age, sex, civil status, years of education, household size, asset index, rural residence and membership to *Oportunidades*.

Table 2.7 *Seguro Popular* intention-to-treat effects on health insurance uptake

	(1) Health insurance all	(2) Health insurance poor
1. Difference-in-differences	0.248***	0.296***
	(0.0246)	(0.0425)
Observations	16,695	5,299
2. Two-way fixed effects	0.0318	0.0443
	(0.0398)	(0.0524)
Observations	8,805	3,734
3. Triple differences	1.624***	1.238**
	(0.483)	(0.515)
Observations	16,695	5,299
4. Event-study model		
year -5	-0.0988	-0.239
	(0.112)	(0.152)
year -4	-0.0982	-0.208*
	(0.0941)	(0.112)
year -3	-0.0607	-0.179*
	(0.0722)	(0.100)
year -2	-0.0252	-0.103
	(0.0574)	(0.0872)
year 0	0.0450	0.0149
	(0.0702)	(0.0861)
year 1	0.161*	0.186
	(0.0937)	(0.125)
year 2	0.174	0.286**
	(0.109)	(0.134)
year 3	0.408***	0.668***
	(0.138)	(0.153)
year 4	0.394**	0.619***
	(0.171)	(0.218)
year 5	0.507***	0.821***
	(0.192)	(0.208)
year 6	0.484**	0.717***
	(0.220)	(0.255)
year 7	0.526**	0.779***
	(0.258)	(0.291)
Observations	8,805	3,734

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes. This table shows intention-to-treat effects of *Seguro Popular* on healthcare utilisation. Panels 1 & 2 are estimations resulted from using equation 2.6; panel 3, uses equation 2.9 and panel 4 are estimations from equation 2.8. All estimations include year and municipality fixed effects and a set of socio-demographic covariates including age, sex, civil status, years of education, household size, asset index, rural residence and membership to *Oportunidades*.

Table 2.8 Descriptive statistics of unmatched and matched samples

	Unmatched sample				Matched sample			
	SP non-recipients		SP recipients		SP non-recipients		SP recipients	
Variable	N	Mean	N	Mean	N	Mean	N	Mean
Socio-demographic								
Age	5393	37.18	2274	38.38	2713	35.74	1347	36.86
Sex (male=1)	5741	0.46	2414	0.43	2813	0.44	1398	0.43
Civil status (married=1)	5775	0.51	2431	0.59	2812	0.57	1398	0.57
Years of education	4468	7.68	2051	6.92	2666	6.7	1344	6.68
Household size	5776	6.21	2431	7.41	2813	7.87	1398	7.58
Asset index	5588	6.26	2357	5.55	2802	5.46	1390	5.46
Rural area	5776	0.7	2431	0.41	2813	0.39	1398	0.36
Oportunidades recipient	5005	0.08	2168	0.21	2778	0.22	1384	0.22
Ever had serious accident?	5775	0.1	2431	0.09	2812	0.09	1398	0.09
Over 50 years old	5775	0.23	2431	0.23	2812	0.2	1398	0.21
Health outcomes								
Body mass index	5156	27.07	2192	27.47	2589	26.65	1290	27.26
Overweight	5156	0.36	2192	0.38	2589	0.34	1290	0.36
Obesity	5156	0.27	2192	0.28	2589	0.24	1290	0.27
Hypertension	5069	0.08	2168	0.08	2477	0.1	1251	0.08
Anaemia	4446	0.13	1935	0.13	2260	0.16	1143	0.14
Self-reported health								
Reports good health	5774	0.58	2431	0.49	2811	0.49	1398	0.49
Left activities for health related reasons last 4 weeks	5773	0.08	2431	0.09	2810	0.1	1398	0.07
Reports health to be better than last year	5765	0.26	2430	0.23	2807	0.24	1398	0.23
Predicts health to be better next year	5774	0.38	2430	0.34	2812	0.35	1398	0.35
Reports good health for age/gender	5774	0.34	2431	0.3	2812	0.29	1398	0.32
Healthcare utilisation								
OP visit to hospital/clinic during last 4 weeks	5763	0.1	2429	0.13	2806	0.12	1397	0.11
OP visit to dr/healer during last 4 weeks	5763	0.01	2429	0.01	2806	0.02	1397	0.01
IP visit to hosp/clinic during last 12 months	5762	0.04	2429	0.05	2807	0.04	1397	0.05
Social security/private health insurance?	5776	0.07	2431	0.32	2813	0.04	1398	0.24

Notes. This table displays a descriptive summary of the sample contained in MxFLS1-3. The matching is conducted by entropy balance. SP recipients refer to the households where at least one member declared being enrolled in SP; non-recipients are households where no one of its members are social security beneficiaries.

Table 2.9 *Seguro Popular* average treatment effects on health outcomes

	(1) Hypertension all	(2) Hypertension poor	(3) Anaemia all	(4) Anaemia poor
1. Difference-in-differences	-0.0251	-0.0536	-0.0674	-0.0919
	(0.0301)	(0.0584)	(0.0815)	(0.111)
Observations	1,078	456	648	280
2. Matched diff-in-diff	-0.0427	-0.0634	-0.0282	-0.0294
	(0.0442)	(0.0754)	(0.103)	(0.106)
Observations	1,078	456	648	280
3. Two-way fixed effects	-0.00812	-0.00412	-0.0229	-0.0189
	(0.0132)	(0.0184)	(0.0186)	(0.0315)
Observations	11,142	4,861	8,128	3,356
4. Matched TW-FE	-0.00381	-0.00465	-0.0224	-0.00647
	(0.0129)	(0.0294)	(0.0192)	(0.0479)
Observations	9,032	3,100	7,682	2,208

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes. This table presents average treatment effects of *Seguro Popular* on health status. Panels 1 & 2 are estimations using equation 2.10 on two MxFLS waves; panels 3 &4, using the same model, extend to three waves. For specifications 2 & 4, data is preprocessed by entropy balance to match affiliates to non-affiliates on observable charafcteristics. All estimations include year and municipality fixed effects and a set of socio-demographic covariates including age, sex, civil status, years of education, household size, asset index, rural residence and membership to *Oportunidades*.

Table 2.10 *Seguro Popular* average treatment effects on self-reported health status

	(1) Health self-assessment all	(2) Health self-assessment poor	(3) Health compared to last year all	(4) Health compared to last year poor
1. Difference-in-differences	0.0333	0.0972	-0.0110	0.0344
	(0.0764)	(0.119)	(0.0697)	(0.0895)
Observations	1,114	470	1,110	470
2. Matched diff-in-diff	0.0480	0.124	0.0271	0.135
	(0.0936)	(0.133)	(0.0833)	(0.116)
Observations	1,114	470	1,110	470
3. Two-way fixed effects	-0.0287	-0.0257	-0.00150	-0.0181
	(0.0211)	(0.0339)	(0.0201)	(0.0313)
Observations	11,527	5,016	11,506	5,003
4. Matched TW FE	-0.0419*	-0.0170	0.0142	0.0175
	(0.0233)	(0.0576)	(0.0185)	(0.0442)
Observations	9,298	3,194	9,288	3,183

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes. This table presents average treatment effects of *Seguro Popular* on self-reported health status. Panels 1 & 2 are estimations using equation 2.10 on two MxFLS waves; panels 3 &4, using the same model, extend to three waves. For specifications 2 & 4, data is preprocessed by entropy balance to match affiliates to non-affiliates on observable charafcteristics. All estimations include year and municipality fixed effects and a set of socio-demographic covariates including age, sex, civil status, years of education, household size, asset index, rural residence and membership to *Oportunidades*.

Table 2.11 *Seguro Popular* average treatment effects on healthcare utilisation

	(1) Outpatient visit in last 4 weeks all	(2) Outpatient visit in last 4 weeks poor	(3) Hospitalisation last year all	(4) Hospitalisation last year poor
1. Difference-in-differences	0.00501	-0.0381	0.00422	-0.000418
	(0.0462)	(0.0750)	(0.0267)	(0.0483)
Observations	1,110	468	1,110	468
2. Matched diff-in-diff	-0.00872	-0.0545	-0.00583	-0.0124
	(0.0517)	(0.0763)	(0.0309)	(0.0496)
Observations	1,110	468	1,110	468
3. Two-way fixed effects	0.0245	0.0244	0.00807	0.00274
	(0.0181)	(0.0252)	(0.00894)	(0.0126)
Observations	11,511	5,011	11,512	5,010
4. Matched TW FE	0.0294**	0.0459	0.0160*	-0.00101
	(0.0146)	(0.0430)	(0.00878)	(0.0210)
Observations	9,289	3,191	9,289	3,190

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes. This table presents average treatment effects of *Seguro Popular* on healthcare utilisation. Panels 1 & 2 are estimations using equation 2.10 on two MxFLS waves; panels 3 &4, using the same model, extend to three waves. For specifications 2 & 4, data is preprocessed by entropy balance to match affiliates to non-affiliates on observable charafcteristics. All estimations include year and municipality fixed effects and a set of socio-demographic covariates including age, sex, civil status, years of education, household size, asset index, rural residence and membership to *Oportunidades*.

Table 2.12 *Seguro Popular* average treatment effects on health insurance uptake

	(1) Health insurance all	(2) Health insurance poor
1. Difference-in-differences	0.695***	0.647***
	(0.0457)	(0.0687)
Observations	1,114	470
2. Matched diff-in-diff	0.708***	0.666***
	(0.0491)	(0.0710)
Observations	1,114	470
3. Two-way fixed effects	0.745***	0.748***
	(0.0147)	(0.0200)
Observations	11,530	5,017
4. Matched TW FE	0.696***	0.737***
	(0.0134)	(0.0260)
Observations	9,298	3,194

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes. This table presents average treatment effects of *Seguro Popular* on healthcare utilisation. Panels 1 & 2 are estimations using equation 2.10 on two MxFLS waves; panels 3 &4, using the same model, extend to three waves. For specifications 2 & 4, data is preprocessed by entropy balance to match affiliates to non-affiliates on observable charafcteristics. All estimations include year and municipality fixed effects and a set of socio-demographic covariates including age, sex, civil status, years of education, household size, asset index, rural residence and membership to *Oportunidades*.

Table 2.13 *Seguro Popular* intention-to-treat effects on infant mortality rates

	(1)	(2)	(3)	(4)
	All		Poor	
	Rate	SE	Rate	SE
years after roll-out				
-6	0.833	(1.456)	3.426	(2.819)
-5	0.446	(1.117)	2.594	(2.114)
-4	0.528	(0.866)	2.612	(1.632)
-3	0.505	(0.598)	1.984*	(1.119)
-2	0.370	(0.299)	1.240**	(0.620)
0	0.0386	(0.289)	-1.048*	(0.596)
1	-0.0822	(0.531)	-2.211**	(1.115)
2	-0.0974	(0.778)	-3.622**	(1.628)
3	0.136	(0.995)	-4.922**	(2.139)
4	0.205	(1.218)	-6.765**	(2.677)
5	0.237	(1.446)	-7.857**	(3.221)
6	0.475	(1.680)	-9.221**	(3.738)
7	0.698	(1.882)	-10.15**	(4.283)
8	1.031	(2.119)	-11.66**	(4.819)
9	1.021	(2.327)	-13.41**	(5.345)
10	1.378	(2.507)	-15.28***	(5.886)
11	1.423	(2.697)	-17.54***	(6.428)
12	1.817	(2.879)	-19.18***	(6.989)
13	1.767	(3.077)	-21.19***	(7.546)
14	1.869	(3.293)	-24.25***	(8.163)
Constant	12.69***	(1.168)	19.61***	(2.383)
Observations	36,619		18,033	
R-squared	0.483		0.452	

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes. This table displays intention-to-treat effects of *Seguro Popular* on mortality rates. Both models are estimated using equation 2.8. Data on mortality is aggregated at the municipality-year level and includes from 2000 to 2017. Both regressions are weighted by number of births in that year by municipality. The sample in column 3 includes only municipalities with high or very high marginalisation index.

Table 2.14 *Seguro Popular* intention-to-treat effects on children under 10 mortality rates

	(1)	(2)	(3)	(4)
	All		Poor	
VARIABLES	Rate	SE	Rate	SE
years after roll-out				
-6	0.0236**	(0.0103)	0.0398*	(0.0239)
-5	0.0179**	(0.00810)	0.0193	(0.0183)
-4	0.0152**	(0.00631)	0.0235	(0.0143)
-3	0.0117***	(0.00441)	0.0175*	(0.00987)
-2	0.00630***	(0.00216)	0.0119**	(0.00534)
0	-0.00341*	(0.00206)	-0.00878*	(0.00511)
1	-0.00747**	(0.00372)	-0.0168*	(0.00951)
2	-0.0110**	(0.00532)	-0.0298**	(0.0140)
3	-0.0126*	(0.00687)	-0.0392**	(0.0184)
4	-0.0150*	(0.00831)	-0.0551**	(0.0231)
5	-0.0181*	(0.00987)	-0.0646**	(0.0277)
6	-0.0195*	(0.0114)	-0.0760**	(0.0325)
7	-0.0207	(0.0127)	-0.0820**	(0.0371)
8	-0.0221	(0.0142)	-0.0967**	(0.0419)
9	-0.0254	(0.0155)	-0.110**	(0.0465)
10	-0.0259	(0.0166)	-0.125**	(0.0511)
11	-0.0275	(0.0177)	-0.138**	(0.0557)
12	-0.0291	(0.0190)	-0.152**	(0.0603)
13	-0.0308	(0.0199)	-0.165**	(0.0649)
14	-0.0331	(0.0215)	-0.183**	(0.0712)
Constant	0.126***	(0.00833)	0.181***	(0.0233)
Observations	38,939		19,621	
R-squared	0.606		0.541	

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes. This table displays intention-to-treat effects of *Seguro Popular* on mortality rates. Both models are estimated using equation 2.8. Data on mortality is aggregated at the municipality-year level and includes from 2000 to 2017. Both regressions are weighted by the population size of each age group. The sample in column 3 includes only municipalities with high or very high marginalisation index.

Table 2.15 *Seguro Popular* intention-to-treat effects on mortality rates of adults over age 20

	(1)	(2)	(3)	(4)
	All		Poor	
VARIABLES	Rate	SE	Rate	SE
years after roll-out				
-6	0.234	(0.296)	-0.341	(0.438)
-5	0.200	(0.224)	-0.213	(0.328)
-4	0.129	(0.170)	-0.130	(0.250)
-3	0.0612	(0.117)	-0.0571	(0.169)
-2	0.0154	(0.0594)	0.00142	(0.0893)
0	-0.0670	(0.0576)	0.0750	(0.0900)
1	-0.119	(0.112)	0.163	(0.167)
2	-0.145	(0.165)	0.176	(0.248)
3	-0.225	(0.222)	0.127	(0.328)
4	-0.257	(0.275)	0.147	(0.410)
5	-0.320	(0.330)	0.111	(0.487)
6	-0.385	(0.380)	0.0644	(0.571)
7	-0.453	(0.433)	0.121	(0.655)
8	-0.533	(0.491)	0.0209	(0.741)
9	-0.554	(0.553)	0.148	(0.825)
10	-0.642	(0.608)	0.0921	(0.903)
11	-0.669	(0.661)	0.0136	(0.975)
12	-0.739	(0.716)	0.0367	(1.054)
13	-0.727	(0.773)	0.142	(1.117)
14	-0.702	(0.831)	0.308	(1.188)
Constant	6.899***	(0.302)	7.241***	(0.406)
Observations	46,561		24,499	
R-squared	0.907		0.788	

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes. This table displays intention-to-treat effects of *Seguro Popular* on mortality rates. Both models are estimated using equation 2.8. Data on mortality is aggregated at the municipality-year level and includes from 2000 to 2017. Both regressions are weighted by the population size of each age group. The sample in column 3 includes only municipalities with high or very high marginalisation index.

Table 2.16 *Seguro Popular* intention-to-treat effects on mortality rates of adults over age 60

	(1)	(2)	(3)	(4)
	All		Poor	
VARIABLES	Rate	SE	Rate	SE
years after roll-out				
-6	-0.714	(1.147)	-1.948	(1.752)
-5	-0.137	(0.868)	-1.583	(1.319)
-4	-0.264	(0.667)	-1.079	(1.015)
-3	-0.355	(0.464)	-0.889	(0.692)
-2	-0.214	(0.231)	-0.169	(0.363)
0	0.0351	(0.232)	0.519	(0.369)
1	0.0911	(0.452)	1.064	(0.672)
2	0.215	(0.655)	1.539	(1.000)
3	0.155	(0.877)	1.817	(1.319)
4	0.164	(1.090)	2.088	(1.634)
5	0.0429	(1.297)	2.392	(1.943)
6	0.161	(1.509)	2.694	(2.250)
7	0.0468	(1.716)	2.866	(2.565)
8	-0.0213	(1.920)	3.102	(2.859)
9	0.135	(2.133)	4.091	(3.162)
10	-0.0860	(2.324)	4.078	(3.455)
11	0.113	(2.525)	4.303	(3.720)
12	-0.0186	(2.714)	4.767	(4.019)
13	0.308	(2.921)	5.548	(4.273)
14	0.532	(3.141)	6.171	(4.540)
Constant	28.68***	(1.158)	25.79***	(1.547)
Observations	46,490		24,452	
R-squared	0.832		0.715	

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes. This table displays intention-to-treat effects of *Seguro Popular* on mortality rates. Both models are estimated using equation 2.8. Data on mortality is aggregated at the municipality-year level and includes from 2000 to 2017. Both regressions are weighted by the population size of each age group. The sample in column 3 includes only municipalities with high or very high marginalisation index.

Chapter 3

Universal healthcare coverage effects on health inequality

Abstract

The objective of the present study is to assess the effect of *Seguro Popular*, a healthcare policy expansion, on childhood health and childhood health inequality. Using non-linear difference-in-differences methods to estimate unconditional quantile treatment effects, this analysis finds that the policy expansion had a mild positive impact on the child height-for-age distribution. Additionally, comparing inequality measures between the estimated and counterfactual health distributions, results show that the policy had a marginal effect reducing childhood health inequalities.

3.1 Introduction

At the beginning of the 21st century in Mexico, the health system was divided in a set of disconnected subsystems where roughly half of the population had no health insurance, and more than half of the health spending was made out-of-pocket. These inequalities in access to healthcare produced deep inequalities in health outcomes. To address this situation, the Ministry of Health implemented a large-scale healthcare expansion to guarantee every individual's constitutional right to healthcare[1].

Seguro Popular, a public health insurance programme was formally implemented in 2004, with the aim of closing the gap in healthcare coverage. By 2000, only half of the population (roughly 50 million people) were recipients of social security with health benefits; the other half were not covered by social security and did not have medical insurance. Moreover, about 60% of healthcare was covered by patients' out-of-pocket payments. *Seguro Popular* was introduced to close the gap in access to healthcare and indirectly improve the health in the population. The programme was progressively implemented at the geographical and population level. Roll-out at the municipality level was completed by 2009, while on the individual level took nearly a decade. The staggered roll-out of the programme offers an external source of variability in treatment exposure among households across municipalities that can be used as an identification strategy.

[1] Ley General de Salud. http://www.ordenjuridico.gob.mx/Documentos/Federal/pdf/wo11037.pdf

There are previous studies on the impact of SP on health status. Programme effects found in early evaluations range from mild to non-existent (Barros et al., 2008; King et al., 2009, 2007) while more recent studies find positive impacts of the programme on population's health status (Conti and Ginja, 2017; Pfutze, 2014). A large part of the literature has focused on the programme's impact on improved access to healthcare and on reducing out-of-pocket healthcare expenditures (Galárraga et al., 2010; Grogger et al., 2015; Knaul et al., 2018; Sosa-Rubí et al., 2011) finding substantial gains from the policy expansion. Previous SP evaluations use the staggered roll-out of *Seguro Popular* as an identification strategy both for cross-section and panel data analyses. In general, literature has focused on finding average effects, but not on distributional impacts.

Using data from two national surveys, this study analyses the impact of SP along the childhood nutritional health distribution and estimates the change in health inequality that resulted from the introduction of *Seguro Popular*. The analysis is conducted in two steps: first, a non-linear difference-in differences method is used to assess children's response to the programme by their change in nutritional status; second, inequality measures in child malnutrition are compared before and after the programme to evaluate the change in health inequality. The hypothesis is that if *Seguro Popular* is reducing the inequalities in healthcare access, then it might be reducing inequalities in health status. By being targeted by *Seguro Popular*, poorest uninsured families – generally with the worst health outcomes – should receive the greatest benefit from the programme.

3.2 Background

3.2.1 Childhood nutrition

Undernutrition refers to the deficiency in a person's intake of energy and/or nutrients. It could be caused by an insufficient calorie intake or by the inability to absorb food nutrients; but according to WHO (1995), it is generally the combination of both: a poor diet and infections that impede the body's ability to assimilate food. The definition includes wasting (low weight-for-height), stunting (low height-for-age) and underweight (low weight-for-age).

Child undernutrition is a global health problem with serious and long-lasting social, economic and health-related consequences. Undernutrition is associated with loss in health and human capital (Black et al., 2013). In a systematic review, Victora et al. (2008) document the link between undernutrition and short adult height, less schooling, reduced economic productivity and, for women, lower offspring birthweight. Moreover, children who are undernourished at an early age and experience rapid weight gain later in childhood, have an increased risk of developing chronic diseases (Uauy et al., 2008). Evidence suggests that nutrition status and specifically height-for-age at age 2 was found to be the best predictor of human capital in later life (Victora et al., 2008); moreover, investments in nutrition during early childhood – before 3 years of age – have the highest impact on human capital (Hoddinott et al., 2008).

By 2019, there were 191 million children undernourished globally, and around 4.5% of childhood mortality was linked to undernutrition (WHO, 2020). Developing countries are especially affected by this problem. Prevalence of wasting and stunting have declined in the recent decades from over 230 million children undernourished in 2000 but continues to be a public health problem especially in low and middle-income countries (WHO, 2020). Undernutrition was a widespread problem in Mexico by the time *Seguro Popular* was introduced. By 1999, the prevalence of stunting was 21.5% while the prevalence of wasting was 2.1% (Shamah-Levy et al., 2020). Child nutrition has improved in the last 2 decades: by 2018, around 14.2% children were stunted and 1.4% wasted. Although there was an significant decline, undernutrition continues to be a public health issue among the poorest populations. Shamah-Levy et al. (2020), highlight the fact that that prevalence of wasting was more than double in rural (2.3%) than in urban areas (1.1%). The objective of this study is to determine whether the improvement in childhood nutrition in Mexico is associated with the introduction of *Seguro Popular*.

3.2.2 Previous literature

Childhood health and nutrition

Health events early in life can have long lasting impacts on health and socio-economic outcomes. Currie and Almond (2011) provide an overview of the relationship between health related negative events or conditions in early childhood and future long term adult outcomes. Health shocks early in life can impair development, however investments on early childhood can revert the damage. Public policy interventions at a young age may help revert a negative shock.

Cash transfers programmes can help reduce the impact of health shocks. Aguero et al. (2006) estimate the impact of an unconditional cash transfer programme on child nutrition using continuous treatment methods. Taking advantage of the exogenous variation in the programme roll-out, Aguero et al. (2006) find that large dosages of Child Support Grant treatment early in life significantly improves childhood nutrition. Behrman and Hoddinott (2001) evaluate the effects of *Progresa* (later renamed as *Oportunidades* and then *Prospera*), a conditional cash transfer programme in Mexico on children's 12 to 36 month height. In addition to households' transfers, children in the treatment group received a nutritional supplement. They found that the programme increased average growth per year in children and reduced the probability of stunting.

Adequate calorie intake can have significant effects on height. Griffen (2016) estimated a height production function using data from a randomised nutrition intervention in Guatemala 1969-1977. Study findings suggest that a 100 calorie increase over a year would increase height by 0.06 cm. Calories explain 16% of height difference between Guatemalan and US born children of Guatemalan immigrants. However, infectious diseases are an hindrance to nutrient absorption. Abramovsky et al. (2019) estimate child health production functions and found that better water, sanitation and hygiene practices make nutrition intake more productive for children of 6-24 months.

Infectious diseases can have a great impact on nutritional health. De Cao (2015) studies the determinants of height from birth to age two using longitudinal data on Filipino children. Results show that diseases play a major role in reducing height. An extra episode of diarrhoea was found to reduce the height of boys by 2 to 2.2 cm and girls by 3.2 to 3.5 cm. Black et al. (2008) estimated that undernutrition and its consequences were responsible of 35% of child deaths.

Antenatal care programmes may also have important impacts on nutritional health. Frankenberg et al. (2005) examine the impact of an expansion of health services in Indonesia which consisted of placing over 50,000 midwives throughout the country on children height-for-age. They exploit the exogenous variation in timing of the reform, the biology of childhood growth and rich longitudinal data. Their results show that nutritional status of children that are fully exposed to a midwife during early childhood (ages 1 to 4) is significantly better than those who are partly exposed cohorts from same communities and same age.

Genetic and environmental factors are also important. Black et al. (2013) examined the effect of maternal stature on childhood grow and found that the risk for children to be stunted was 19.4% for the tallest mothers and 68.2% for the shortest. The risk for offspring to be stunted and underweight was the lowest for the tallest mothers ($\geq$160 cm), the risk increased for shorter mothers and the children whose mothers were under 145 cm had the highest risk.

While public health insurance may help close the gap, it might not be enough. Currie (1995) examines the impact of Medicaid on children and finds that public health insurance narrows the socio-economic gap in utilisation and health among children. However, inequalities and inefficiencies persist, suggesting universality and outreach are important components in health systems.

Seguro Popular

A large part of research on *Seguro Popular* has focused on the programme's impact on health insurance coverage, use of health care services, and household spending on health care. Other studies have found that *Seguro Popular* affiliates are healthier than non-affiliates, using both cross-sectional data (Sosa-Rubi et al., 2009) and repeated cross sections (Conti and Ginja, 2017).

A randomised study (King et al., 2009, 2007) was designed and implemented with cooperation from the Mexican government when *Seguro Popular* was first introduced to estimate its impacts on health care utilisation, health status, and financial protection ten months after introduction. The study found no effects on the outcomes of interest. Moreover, there are external validity concerns – the experiment was run in a small number of carefully selected municipalities which were unlikely to be representative of the entire country; and only covers ten months – arguably not enough time for many health outcomes to respond to roll-out of health insurance. Additionally, using the original experimental data complemented with administrative data, Spenkuch (2012), finds that programme affiliates are less likely to use preventive health care services, a finding that suggests moral hazard among beneficiaries. On the other hand, there is growing evidence that *Seguro Popular* does reduce out-of-pocket health care expenditure (Galárraga et al., 2010; Grogger et al., 2015; Knaul et al., 2018).

Specifically on childhood health outcomes, Conti and Ginja (2017) exploit the staggered timing of implementation of SP by comparing changes in mortality for all age groups in municipalities that introduced it in different years between 2002 and 2010, and find a reduction on mortality of 10% among infants living in poor municipalities. They also find that the introduction of SP in poor municipalities is associated with an immediate 7% increase in obstetric-related hospital admissions and with a 6% increase in overall hospital admissions.

Pfutze (2014) using data from the Mexican Census, estimates the impact of *Seguro Popular* on infant mortality during the first 5 years operating by a weighted probit approach correcting for selection bias and finds a decline in child mortality of 5 per 1000 infants.

Celhay et al. (2019), analyse the effects of the Medical Insurance Century XXI (SMSXXI) a component of *Seguro Popular* introduced on December 1st, 2006 with the aim to reduce out-of-pocket healthcare expenditures on children under 5 years old from families with no social security benefits. Families of enrolled children were automatically enrolled in *Seguro Popular*. SMSXXI gradually expanded its health benefits from 108 interventions and procedures in 2006 to 150 in 2018, as well as its geographical and population reach; although, by 2016 10% of the target population was still not covered (Celhay et al., 2019). In a comprehensive analysis of SMSXXI, the authors examined several data sources using multiple econometric approaches to assess policy impacts on health outcomes, healthcare utilisation and financial protection. They used the timing variation on the geographic implementation as well as the expansion on the interventions covered as the identification strategy to estimate the programme's effects. The programme was found to have an effect on reducing infant mortality by 5% and late neo-natal mortality by 7%. These results are consistent with those found by Conti and Ginja (2017). Moreover, the mothers of enrolled children of 21 to 67 months were 7.2 percent points more likely to report better health, 14.6 percent points less likely to report episodes of influenza and 4.8 percent points less likely to report diarrhoea episodes. Another important finding was an increase on height-for-age z-scores of 0.052 SDs (0.434 cm) in primary school children covered by insurance after 8 years of implementation. Children of more vulnerable areas had higher benefits, gaining 0.181 SDs (0.879 cm). Previous studies do not find an effect of *Seguro Popular* in height-for-age z-scores: Turrini et al. (2016) show modest effects if any. However, Celhay et al. (2019) examine children in 2015, 8 years after implementation, compared to 2012 used in the other studies. It is important to note that Celhay et al. (2019) estimate intention-to-treat effects by assessing impacts on the eligible population not specifically on beneficiaries. The fact that the SMSXXI eligible population is also SP-eligible makes it impossible to disentangle ITT effects of each programme separately. Moreover, the authors do not provide a close examination of possible declining pre-trends of mortality rates. Descriptive diagrams seem to show a decreasing rate before implementation. The authors provide a falsification test by shifting the implementation start by one year, but given all considerations described, a close look at pre-trends would be more adequate.

Only one study (Turrini et al., 2016) looked at childhood nutritional status. They find that *Seguro Popular* had, at best, modest impacts on nutritional status. According to the authors, supply-side obstacles may be important impediments for the programme to succeed. They use the same data as the current study to estimate average intention-to-treat effects on similar outcomes but with a different identification strategy: they do not take advantage of the panel design of the survey data and compare different children across waves. In contrast, the present study follows children through the window period where they could have benefited from the programme.

Most of previous studies focus on estimating the average effects of the programme; this analysis considers more important to understand the distributional effect of *Seguro Popular* than the mean impact. The particular interest in this analysis is the policy's effect on the lower tail of the distribution: the children with worst nutritional status.

3.3 Methods

3.3.1 Data

The data comes from two different household surveys from Mexico: the Mexican Family Life Survey (MxFLS), and the *Encuesta Nacional de Salud y Nutrición* (ENSANUT) or National Health and Nutrition Survey. The first one is a panel survey that spans the temporal and spatial roll-out of SP; while the second is a cross-sectional survey with five waves collected between 2000 and 2018.

The first wave (MxFLS1) was conducted in 2002, before the official implementation of SP; the second (MxFLS2) in 2005-2006, one year after officially implementing; and the third wave (MxFLS3) in 2009-2012, between 7 and 10 years after roll-out. The baseline sample was representative at the regional, rural-urban, and national levels. It contained data on 35,677 individuals in 8,440 households, located in 150 communities spread over 16 states. The second wave contacted close to 90% of the original sample from 2002 including population who changed residence nationally or internationally. The sample size in MxFLS2 expanded to 40 thousand people, due to the additions on interviewer's families. The third round followed the sample in the previous two rounds and reached close to 90% of the original sample.

MxFLS includes extensive information on health-related outcomes including self-reported health, illness, use of health care, healthcare expenditures, mental health, maternal health and infant health outcomes, as well as anthropometric measures and biomarkers including cognitive ability. The survey also contains detailed information on socioeconomic status on the household and on the individual level. MxFLS included information on whether each household member older than 14 received benefits from social programmes and on health insurance coverage.

Main results obtained with data from MxFLS, are then verified using ENSANUT, a probabilistic national survey with state rural and urban strata representativeness (Romero-Martínez et al., 2013). It was collected in 2002, 2006, 2012 and 2018 with the main focus on the health and nutrition status of the population, their determinants, as well as indicators of access to healthcare (Abúndez et al., 2006). It includes information on household features, socio-demographic information of household members, detailed information on health conditions through antropometric measures, biomarkers, biological samples, immunisation records, diagnostic tests, among others.

The current study, used the 2006 and 2012 editions of the survey for the analysis. ENSANUT 2006 contains data on 48,304 households with 206,700 individuals; of which 24,098 are children, 25,166 teenagers, 45,446 adults; with 90,267 anthropometrics. The 2012 edition included information from 50,528 households and 96,031 individuals with complete data, of which 28,209 are children, 21,519 adolescents, 46,303 adults aged 20 or older, and 14,104 ambulatory health services users.

Additionally, the information used to construct an indicator of the roll-out at the municipality level comes from an administrative dataset that includes the total number of individuals enrolled to the programme at the end of each year from 2002 to 2019.

3.3.2 Outcomes

Children's nutritional status is commonly measured based on anthropometric indicators such as height-for age, weight-for-age and weight-for-height. These indicators are obtained by comparing height and weight measurements to reference curves and each indicator provides different information on the process or outcome of nutrition. This study uses height-for-age as a measure of linear growth and as an assessment of long-term nutrition.

Anthropometric indices are usually expressed in z-scores, defined as the difference between the value for an individual and the median value of the reference population for the same sex and age (or height), divided by the standard deviation of the population. Z-scores express the anthropometric index as a number of standard deviations below or above the mean or median reference value. Indicators expressed as z-scores have the advantage that they can be used to estimate summary statistics.

Height-for-age refers to long-term linear growth and deficits may reflect chronic inadequacies in nutrition or health. Low health-for-age values compared to the median child of same age and gender of reference, indicate "shortness". Weight-for-height is an indicator of current nutritional status and its short-term changes; while weight-for-age is used to monitoring growth and to assess changes in the magnitude of malnutrition over time. In any anthropometric indicator, the cut-off to define malnourishment is generally z-score $= -2$. Values under -2, or two standard deviations below the reference median reflect "stuntedness" or pathological shortness. Stuntedness refers to failure in reaching growth potential (O'Donnell et al., 2007).

3.3.3 Empirical strategy

Identification strategy

This study compares difference-in-differences results from two different surveys. In the MxFLS survey, health outcomes come from three waves: the first was collected in 2002, the second corresponds to 2005 and 2006 and the third wave was conducted from 2009 to 2012. The programme's roll-out started on 2002 as a pilot phase at the municipality level and it was officially implemented in 2004 in stages. By 2008, all the municipalities in the sample were participating in the programme. This means that the first wave was collected before most of the municipalities implemented the programme, in fact only around 7% of municipalities were taking part; by the second wave, roughly three quarters of the sampled municipalities were participating but only 12% of the households in the survey declared to be enrolled; and by the third wave, all sampled municipalities were participating and 42% of households reported to be beneficiaries of the programme. The two editions of the ENSANUT survey used here are from 2006 and 2012, which means both are from the after-treatment period.

Considering that childhood nutrition interventions have the most impact on ages below 4, and almost all gains on nutrition occur in the first 60 months of age (Frankenberg et al., 2005), children's height-for-age would ideally be examined before that window period and at a later stage when they are over 60 months of age. Being exposed to the programme can occur in two ways: if the municipality of residence offers the programme when the child is under five, in which case the parameter to estimate is the intention-to-treatment effect; or if the child's household head reported being affiliated to SP during the window period, in that case, the relevant parameter would be the average treatment effect on the treated. This study uses the first definition of exposure – municipality-level affiliation –as the main strategy to estimate treatment effects; and individual-level affiliation is used to complement the analysis as an alternative strategy.

Municipality-level-affiliation As selection into participation is voluntary, people who decide to enrol into *Seguro Popular* may be different to people that choose not to participate in ways that are not observable from the data. For example, eligible people who choose not to enrol may expect higher benefits from enrolling than those who choose not to enrol. In order to reduce the self-selection bias, intention-to-treat effects are estimated by comparing the outcomes of children living in municipalities that participate in the programme to children from municipalities that do not participate, regardless of household enrolment status.

Using information of affiliation on municipality level, the treated population is defined as children who were under 2 years of age when the municipalities of residence implemented the programme. According to childhood growth biology, a healthcare intervention would have the highest effect on that window period. Children who were 3 years old or more when their municipality got affiliated were the control population. The programme's impact is estimated by taking differences in height between treated and untreated children of similar age before and after treatment. This means comparing treated and controls before five in the pre-treatment period and after five years old in the following period. This design controls for the differences in height due to differences in age and for any differences between treated and untreated areas at the baseline. The remaining differences in height would be the effect of the exposure to SP, given that the parallel trends assumption hold. In this context, it means that, in absence of treatment, the differences in outcomes between groups remains constant in time.

Linear differences-in-differences

The first estimation of treatment effects follows a simple difference-in-differences approach. Programme's intent-to-treat effects on children from participating municipalities are estimated and compared to children from eligible households (without social security) but from non-participating municipalities. As participation is voluntary, there may be positive selection as affiliates potentially have unobservable differences with respect to people that choose not to participate. For that reason, and taking into account the biology of childhood growth, the definition of treated population are children whose municipality of residence implemented the programme when they were 2 years old or younger, regardless if their household decided to enrol in *Seguro Popular*. This definition ensures that treated children were exposed to the programme in the window period in which healthcare has the most impact. Intention-to-treat effects are estimated by taking the differences in enrolment over time (pre- and post-treatment), using a model specified as:

$$y_{imt} = \alpha t_t + \gamma g_i + \tau SP_{imt} + x'_{imt}\beta + \eta_i + \pi_m + \delta_t + \theta_{st} + \varepsilon_{it} \tag{3.1}$$

where y_{imt} refers to height-for-age z-score of child i from municipality m at time t; αt_t are the time-specific group-invariant effects; γg_i are group-specific time invariant-effects; the programme effect is the coefficient τ of the interaction SP_{imt} between group and time; the covariates x_{imt} are municipal characteristics, household and individual socio-demographic confounders, plus child gender and age in months; additional controls are individual fixed effects η_i, municipality fixed effects π_m, year fixed effects δ_t, and state-year trends θ_{st}; and finally, ε_{it} is a random error term that captures all omitted factors.

Non-linear differences-in-differences

The interest of this analysis is in the impact of *Seguro Popular* on the entire health outcome distribution. In contrast to average effects, distributional effects allow to assess programme impacts in the presence of heterogeneous effects according to outcome levels. It is reasonable to believe that a programme like *Seguro Popular* that targets the poor – generally the population with the worst health outcomes, could have different effects on different quantiles of the health distribution. Children with the worst health status would probably benefit the most from *Seguro Popular* and therefore, will have a larger change in outcomes. The programme effect on children's health inequality is assessed by comparing the distribution of height-for-age for treated and control populations.

Let F_Y^I be the distribution of height-for-age z-scores of treated children in time t; and F_Y^N, the distribution of control children at time t, where $t = 0$ means pre-programme period and $t = 1$, post-programme period. The quantile treatment effect (QTE) for any quantile $\tau \in (0, 1)$, of a randomly assigned programme, would be the difference at the τth quantile of the treated children's health distribution and the distribution in controls for a given year. However, in a natural experiment setting, we need to construct a counterfactual distribution of outcomes that would be the outcome of the treated on the second period had they not been treated. The treatment effect would be the difference between the distribution of outcomes of the treated population at the second period and the counterfactual distribution.

The treatment effects are estimated using the changes-in-changes method, a non-linear difference-in-differences design proposed by Athey and Imbens (2006) and extended by Melly and Santangelo (2015) to allow for covariates. The method takes the change in the distribution of the comparison group from before to after treatment to estimate the counterfactual, i.e. the distribution of the treatment group in the absence of treatment.

A child belongs to group $G = \{0, 1\}$ where group 1 is the treatment group and is observed in periods $\tau \in \{0, 1\}$. We assume that only children in group 1 and time 1 are affected by the policy. We also observe a vector of covariates X. Following Melly and Santangelo (2015) and Athey and Imbens (2006), the realised outcome is defined as:

$$Y = (1 - I) \cdot Y^N + I \cdot Y^I \tag{3.2}$$

Where Y^N denotes the outcome if the individual does not participate in the programme, and Y^I if the individual participates. In the two-period standard model,

$I = G * T$. Such that, $I = 1$ only for the treatment group on period 1. $I = 0$ for comparison group $G = 0$ and/or for period 0 $T = 0$. The outcome of an individual in the absence of treatment satisfies,

$$Y^N = h(X, T, U) \tag{3.3}$$

Where U is the unobservable component of Y and the production function $h(t, x, u)$ is strictly increasing in u for $t \in \{0, 1\}$ and for all $x \in \mathbb{X}$. The identifying assumption of this model is that the distribution of unobservable characteristics U within groups is constant over time. This assumption is analogous to the parallel trends assumption in the linear DiD design. We need similarity of distribution over time to be able to extrapolate ranks from one period to another. We can identify the conditional distribution Y^N_{11x} over the quantiles $0 < \tau < 1$ as:

$$F^{-1}_{Y^N|11_x}(\tau) = F^{-1}_{Y|01_x}\left(F_{Y|00_x}\left(F^{-1}_{Y|10_x}\left(\tau\right)\right)\right) \tag{3.4}$$

Intuitively, equation 3.4 states that an individual at the τ quantile of the outcome distribution in period 0 and group 0 would be at the $F_{Y|00_x}\left(F^{-1}_{Y|10_x}\left(\tau\right)\right)$ quantile of the outcome distribution in the same period if it were in group 1. The conditional quantile treatment effect for group 1 in period 1 for the covariates x:

$$\Delta^{QE}(\cdot|x) = F^{-1}_{Y^I|11_x}(\cdot) - F^{-1}_{Y^N|01_x}(\cdot) \tag{3.5}$$

$$= F^{-1}_{Y^I|11_x}(\cdot) - F^{-1}_{Y|01_x}\left(F_{Y|00_x}\left(F^{-1}_{Y|10_x}\left(\tau\right)\right)\right) \tag{3.6}$$

However, conditional treatment effects are not useful in this context and given that the coefficients can be difficult to interpret, we are interested in the unconditional treatment effects. The unconditional distribution of outcomes for group 1 in period 1 is obtained by integrating the conditional outcome distribution over the distribution of covariates.

$$F_{Y^I|11}(y) = F_{Y|11}(y) = \int_{\mathbb{X}} F_{Y|00_x}(y) dF_{X|11}(x) \tag{3.7}$$

$$= \int_{\mathbb{X}} F_{Y|10_x}\left(F^{-1}_{Y|00_x}\left(F^{-1}_{Y|01_x}\left(y\right)\right)\right) dF_{X|11}(x) \tag{3.8}$$

The unconditional treatment effects on the potential outcome are given by:

$$\Delta^{QE}(\cdot) = F^{-1}_{Y^I|11}(\cdot) - F^{-1}_{Y^N|11}(\cdot) \tag{3.9}$$

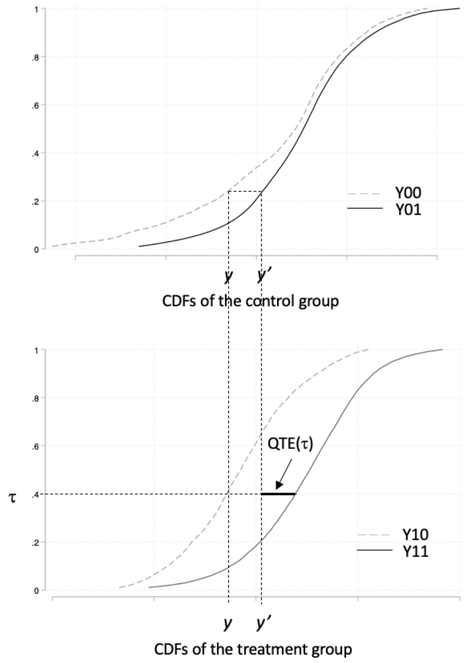

Fig. 3.1 Changes-in-changes distribution transformations

Figure 3.1 illustrates the estimation of a counterfactual distribution: for any quantile, we find the level of outcome y for the distribution of the treated before treatment; then, we find the quantile associated to that level of y on the control group before treatment distribution, and the outcome level y' on the controls after treatment for that quantile.

These results could provide evidence about any differences in inequality derived from the implementation of *Seguro Popular* by looking at the effects on different quantiles. We can also calculate inequality indices using the distributions estimated. In this study, the change in inequality is estimated by comparing the Gini coefficient of the outcome distribution for the treated group on the after-treatment period and the Gini coefficient counterfactual distribution.

Alternative estimators

To perform a further sensitivity analysis of the methods used, estimations are performed using an alternative non-linear DD estimator. Quantile DD (QDD) is similar to CIC as it uses the change in the comparison group health distribution to construct a counterfactual distribution of treated children post-programme in the absence of treatment. However, their assumptions differ. QDD assumes that the counterfactual distribution is:

$$F_1^{-1}\Bigg(F_0(y)\Bigg)_{QDD} = y + \Bigg[G_1^{-1}\Bigg(F_0(y)\Bigg) - G_0^{-1}\Bigg(F_0(y)\Bigg)\Bigg] The QDD effect is given by the double differ \tag{3.10}$$

QCIC transformation differs from QDD, as depicted in Figure 3.1. The parallel trends assumption in QDD means that quantile τ in the treated distribution in absence of treatment would have follow the same trend to quantile τ in the controls. In comparison, the assumption in CIC is less restrictive: quantile τ corresponding to y in the treated distribution is assumed to have evolved similarly to quantile τ' in the controls corresponding to the same level of y. In that way, the population on each side of a level of health y can be different, which allows for heterogeneity in the distribution of unobservables (Athey and Imbens, 2006).

3.4 Empirical results

3.4.1 Municipality level roll-out

Descriptive statistics

Table 3.1 shows descriptive statistics of both samples of treated and controls. The treated population are children who were living in a municipality that offered SP during their 4 first years of age. The mean value of height-for-age z-score is the same for treated and controls, however the distribution is slightly skewed to the left in the treated sample. Controls are 10 months older on average; boys represent slightly above half of the population in both samples. Households in rural areas are 54% in controls and 57% in treated; parents' education was slightly higher in treated population; households had 6.65 members in average for controls and 6.47 for treated. Treated households were richer and parents' were taller on average. The marginalisation index is defined from -3, the lowest marginalisation value to +3 the highest. The treated sample in average lived in less marginalised areas compared to controls.

Table 3.2 describes the sample from the ENSANUT repeated cross sectional dataset. Height-for-age z-score was lower on average on the treated sample. Controls are younger by definition, they were born after the programme was implemented, while treated children were born before roll-out. Half of treated and controls were boys; parent's height and education were similar; household size was slightly greater for controls; mean income decile was greater on treated but both groups had similar median. On average, 65% of treated population lived in rural areas, while only 48%

Table 3.1 Descriptive statistics of treated and controls (MxFLS)

	Controls			Treated		
Variable	N	Mean	Median	N	Mean	Median
Height-for-age z-score	4,219	-0.64	-0.64	1,840	-0.64	-0.61
Age in moths	4,934	61.52	59	2,164	51.78	54
Sex (boys=1)	4,934	0.51	1	2,164	0.52	1
Mother's height	4,798	155.09	155.5	2,098	157.37	157.2
Father's height	3,912	166.93	167	1,733	168.59	168.5
Parents' education	4,759	7.12	6	2,101	7.45	8
Household size	4,934	6.65	6	2,164	6.47	6
Asset index	4,662	5.49	5.82	2,099	5.83	6.12
Rural area	4,934	0.54	1	2,164	0.57	1
Marginalisation index	4,933	-0.84	-0.96	2,164	-1.04	-1.27

Notes. This table shows descriptive statistics of MxFLS data. The treated population are children up to 9 years old who were exposed to *Seguro Popular* from 0-4 years of age.

of controls did. Treated population lived in slightly less marginalised areas in average, however the medians of both populations are similar.

Table 3.2 Descriptive statistics of treated and controls (ENSANUT)

	Controls			Treated		
Variable	N	Mean	Median	N	Mean	Median
Height-for-age z-score	16,524	-0.57	-0.58	19,488	-0.65	-0.64
Age in moths	16,524	72.88	77	19,488	45.49	42
Sex (boys=1)	16,524	0.5	1	19,488	0.5	0
Parents' height	12,843	157.3	156.6	15,280	157.68	157
Parents' education	14,088	2.12	2	16,421	2.17	2
Household size	16,524	5.34	5	19,488	5.2	5
Income decile	16,524	3.82	4	19,488	4.99	4
Rural area	16,524	0.48	0	19,488	0.65	1
Marginalisation index	16,524	-0.78	-1.03	19,488	-0.85	-1.07

Notes. This table shows descriptive statistics of ENSANUT data. The treated population are children who were born before *Seguro Popular* was implemented.

Figure 3.2 displays the evolution of height-for-age z-scores by age of treated and controls for different percentiles for both surveys. Here, treated refers to all children up to 9 years old exposed to the programme from under 2 years old while control refers to all children of 0 to 9 years old exposed to the programme after the age of two. In the MxFLS panel, treated children, represented with the dashed line had a rise from age 6 in average and in all percentiles shown. The increase persist to age 9 and maybe longer in the lower percentiles (P10, P25 and P50) and declining after 8 years old until matching the controls for percentiles P75 and P90. This could be an indication that the programme had a higher effect on children with worse nutritional status. The cross sectional survey in the lower panel, shows that, on average, z-scores of the treated and control groups follow a very similar path throughout the different percentiles.

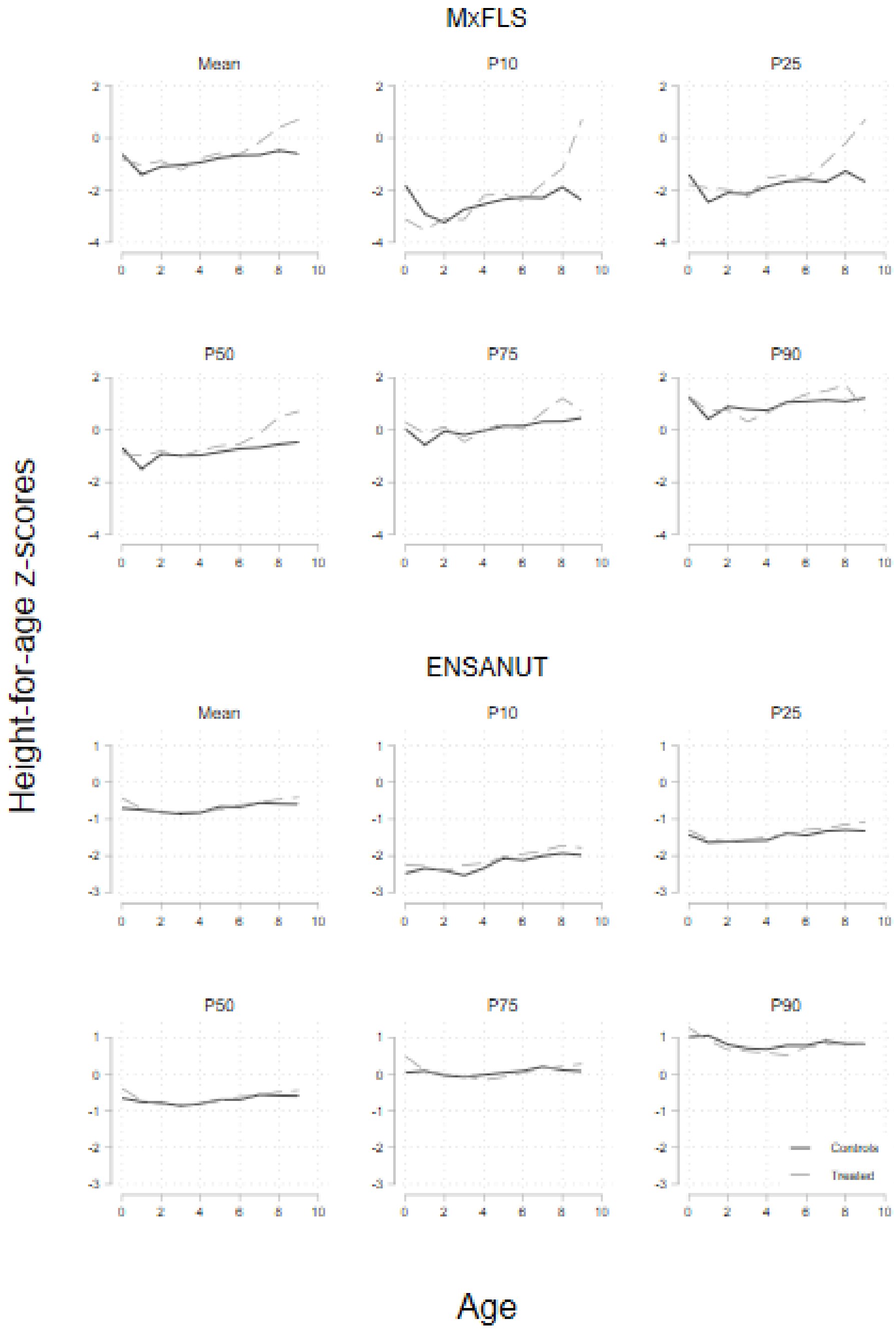

Fig. 3.2 Percentiles of height-for-age z-scores

Programme impacts

Table 3.3 shows the difference-in-differences estimations of mean effects of SP on childhood height-for-age using the model described by equation 3.1 on data from the MxFLS three waves. There was no impact of the programme on children's height-for-age z-scores on average. Differences in height are explained by children's age and sex and fathers' height, household size and asset index. The effect of household size and socioeconomic status measured with an asset index were absorbed by individual, municipality and year fixed effects.

Table 3.3 Intention-to-treat effects of SP on height-for-age scores (MxFLS)

VARIABLES	(1)	(2)	(3)	(4)
Intention-to-treat	0.0390	0.0417	0.124	0.0860
	(0.0728)	(0.0793)	(0.119)	(0.138)
Age in months		-0.00211	-0.0974***	-0.102***
		(0.00142)	(0.0111)	(0.0114)
Sex (boys=1)		-0.0760*	-0.421**	-0.400**
		(0.0417)	(0.193)	(0.197)
Mother's height		0.0364***	-0.00241	-0.00287
		(0.00361)	(0.0297)	(0.0334)
Father's height		0.0310***	0.0505**	0.0549*
		(0.00368)	(0.0233)	(0.0280)
Parent's education		-0.00977		
		(0.00845)		
Household size		-0.0316***		
		(0.0107)		
Asset index		0.138***	0.0444	0.0517
		(0.0265)	(0.0627)	(0.0566)
Rural area		-0.0566	0.148	0.306
		(0.0597)	(0.293)	(0.211)
Marginalisation index		-0.0382		
		(0.0472)		
Individual FE			X	X
Year FE			X	X
Municipality FE			X	X
State x year				X
Constant	-0.766***	-11.95***	-3.484	-3.979
	(0.0623)	(0.834)	(7.510)	(8.642)
Observations	6,058	4,419	3,753	3,739
R-squared	0.010	0.167	0.732	0.753

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.4 shows intention-to-treat effects of SP on height using the data from ENSANUT 2006 and 2012. From the four models, only the second shows any impact; however, when controlling for year and municipality fixed effects, the impact disappears. All the effect is absorbed by the parent's height, number of people living in household and income decile. The cross sectional structure of the survey data, does not allow for individual fixed effects. Data from the panel survey allows to control for self-selection and reducing bias.

Table 3.4 Intention-to-treat effects of SP on height-for-age z-scores (ENSANUT)

VARIABLES	(1)	(2)	(3)	(4)
Intention-to-treat effects	-0.00863	0.0643*	0.0383	0.0330
	(0.0325)	(0.0335)	(0.0331)	(0.0337)
Age in moths		-0.00108**	-0.000604	-0.000622
		(0.000497)	(0.000505)	(0.000509)
Sex (boys=1)		-0.0123	-0.0131	-0.0133
		(0.0152)	(0.0152)	(0.0152)
Parents' height		0.0270***	0.0217***	0.0217***
		(0.000992)	(0.000928)	(0.000931)
Parents schooling		0.0349**	0.0193	0.0183
		(0.0171)	(0.0169)	(0.0168)
Household size		-0.0522***	-0.0410***	-0.0406***
		(0.00521)	(0.00513)	(0.00509)
Income decile		0.0406***	0.0501***	0.0520***
		(0.00443)	(0.00454)	(0.00455)
Rural area		0.0439*		
		(0.0231)		
Marginalisation index		-0.176***		
		(0.0142)		
Year FE			X	X
Municipality FE			X	X
State x year trends				X
Constant	-0.804***	-5.033***	-4.200***	-4.204***
	(0.0289)	(0.155)	(0.150)	(0.151)
Observations	27,926	21,555	21,551	21,551
R-squared	0.006	0.128	0.207	0.208

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

While these mean impacts of the programme are informative, there may be heterogeneous effects through height distribution. The particular interest of this analysis is on the programme effects on the lower tail of the distribution, the children with the worst nutritional health status. In contrast with the lack of impact on height-for-age z-scores shown in Table 3.3, the quantile treatment effects displayed in

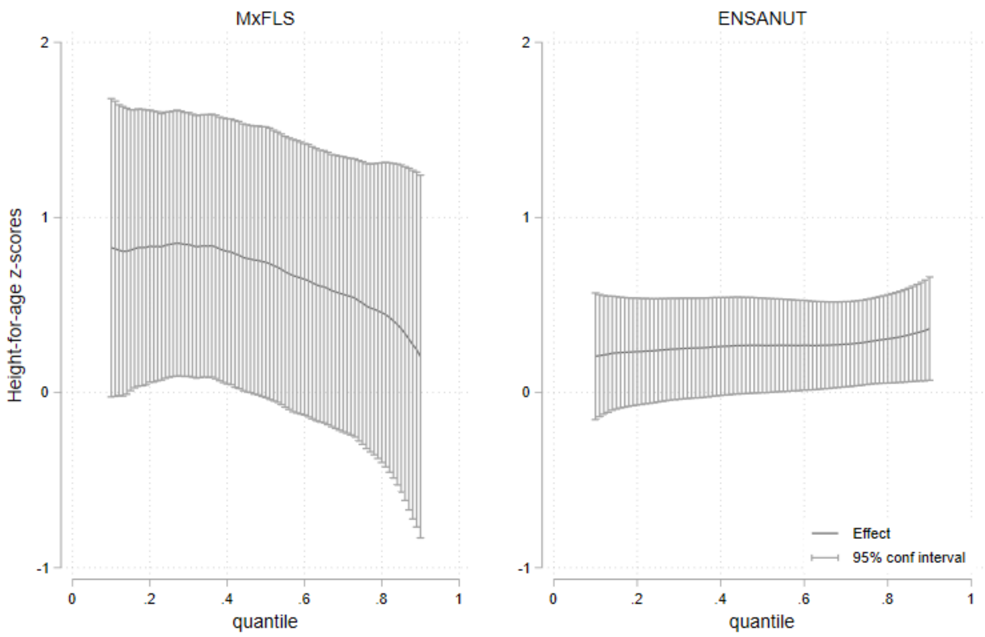

Fig. 3.3 Quantile treatment effects and 95% confidence intervals

the left hand side panel of Figure 3.3, show a significant impact. Moreover, the effect is higher in the lower segment of the distribution, between the .2 and .5 quintiles, implying a greater effect on the children with worse nutriotnal health. The right hand side panel, using the ENSANUT dataset, in comparison, shows a mild positive effect of less than half standard deviation in the z-score; however, the impact is greater on the higher tail of the distribution implying an improvement on the better-off. The discrepancy in the results is due to the difference in dataset structures. In this case the panel dataset, even though less precise, due to the lower sample size, provides less biased results.

Table 3.5 reports both the Kolmogorov-Smirnov and the Cramer-von-Mises-Smirnov statistics. The first null hypotheses tests that there is no effect at all; and the second, if those effects are constant. There is evidence of a marginal treatment impact different than zero on height-for-age in both cases, MxFLS and ENSANUT; and there is evidence of constant effects for different quantiles. Moreover, there is evidence of stochastic dominance illustrated on Figure 3.4 showing the counterfactual and the empirical cumulative distributions. The counterfactual is the hypothetical outcome distribution of the treated if they had not been treated. The treatment effects are represented by the horizontal distance between those curves for each quantile. The counterfactual distribution is always to the left of the empirical distribution for both the cross-sectional sample and on the panel data survey, which means positive

Table 3.5 Statistical tests of treatment effects and stochastic dominance

Null-hypothesis	MxFLS		ENSANUT	
	KS-statistic	CMS-statistic	KS-statistic	CMS-statistic
No effect: QTE(tau)=0 for all taus	0.02	0.01	0.01	0.01
Constant effect: QTE(tau)=QTE(0.5) for all taus	0.21	0.26	0.42	0.73
Stochastic dominance: QTE(tau)>0 for all taus	1	1	1	1
Stochastic dominance: QTE(tau)<0 for all tau	0.01	0.0	0.0	0.0

effects of the programme. These results suggest that the roll-out of *Seguro Popular* did reduce health inequalities using the MxFLS data as the improvement was higher for the children in the lower segment of the distribution (i.e. lowest height-for-age z-score); however, according to the analysis on the ENSANUT dataset, inequality incrased as impacts are greater on the upper end of the distribution (i.e. children with greater height-for-age z-score). The next section examines the change in health inequality resulting from the introduction of *Seguro Popular*.

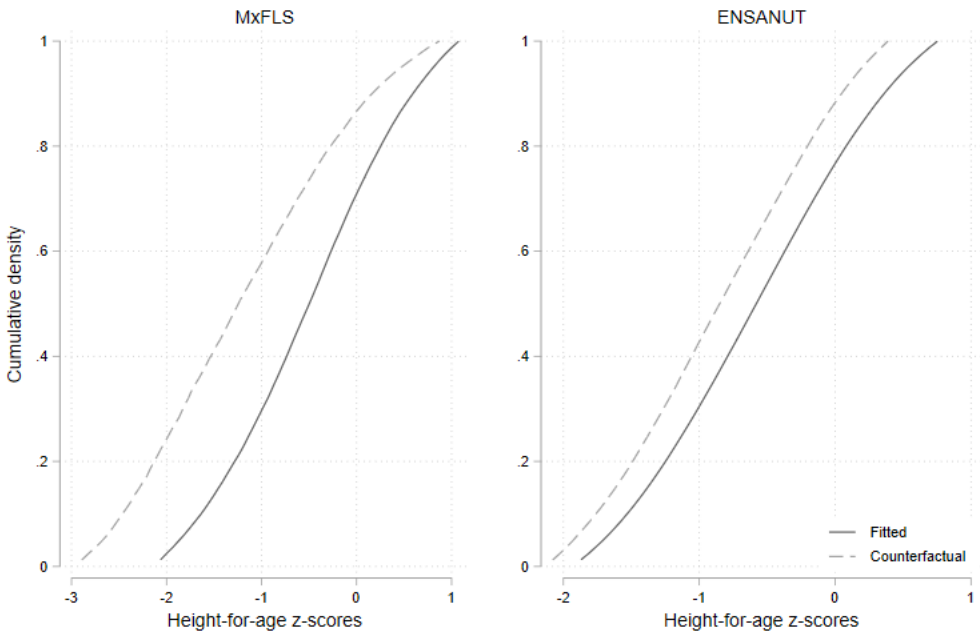

Fig. 3.4 CIC estimated and counterfactual distributions

Changes in inequality

Health inequality has been defined as variations in health across individuals in the population (WHO, 2000). This study is interested in the change in health inequality in the distribution of nutrition status across individuals resulted from the introduction of SP. Those changes are explored by calculating Gini coefficients of the treated and

Table 3.6 Generalised Gini coefficients of CIC distributions

		MxFLS		
Distribution	N	Index value	Std error	p-value
Counterfactual	81	0.55578867	0.00602909	0.0000
Estimated	81	0.46362859	0.00453672	0.0000
Test for stat. significant differences H0: diff=0				
F-stat	p-value			
149.18708	0.0000			
Test for stat. significant differences H0: diff=0				
Diff	Std. error	z-stat	p-value	
-.09216008	.00754531	-12.21	0.0000	
		ENSANUT		
Distribution	N	Index value	Std error	p-value
Counterfactual	81	0.45226353	0.00287878	0.0000
Estimated	81	0.69722972	.0053918	0.0000
Test for stat. significant differences H0: diff=0				
F-stat	p-value			
1606.2708	0.0000			
Test for stat. significant differences H0: diff=0				
Diff	Std. error	z-stat	p-value	
0.24496619	0.00611219	40.08	0.0000	

counterfactual distribution at the post-treatment period. The Gini coefficient is a relative measure that compares each individual's health to every other individual's health. It ranges from 0 (perfect equality) to 1 (maximum inequality). Inequality can be reduced by increasing the health status of the sickest by a larger magnitude than the healthiest.

Table 3.6 shows the change in Gini coefficients. On the upper panel, using the MxFLS dataset, the Gini coefficient of the counterfactual distribution obtained by the changes-in-changes model was 0.09 greater than the Gini coefficient of the estimated distribution. This means that there is a decline in inequality as those with relatively lower health status (i.e. lower height-for-age) had the greatest effect comparatively. Moreover, the difference in inequality is statistically significant. In contrast, on the lower panel, the Gini of the estimated distribution is 0.24 higher than that of the counterfactual, which means an increase in inequality which is statistically significant. Main results from the panel survey MxFLS provide concluding evidence of *Seguro Popular* effect on reducing health inequality. The longitudinal structure of the data allows to reduce selection bias controlling for individual unobserved characteristics to a greater degree than the strategy using cross sectional database that compares different children across time.

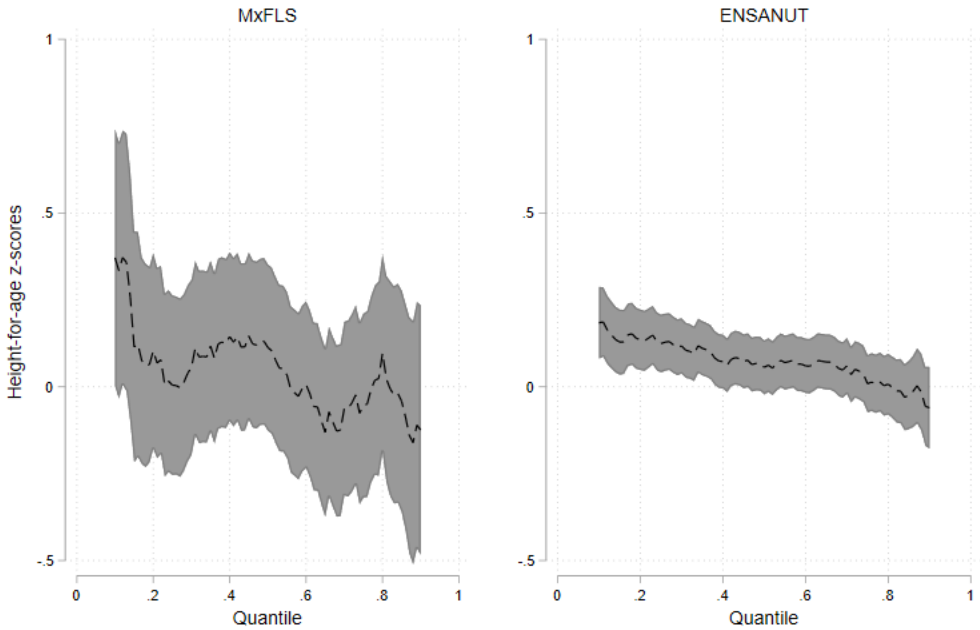

Fig. 3.5 QTE of alternative non-linear DiD estimators: QDD

Alternative estimators

Figure 3.5 shows the quantile treatment effects of SP on the nutritional status of children using the QDD estimator for both survey datasets. The difference in assumption between models lead to different results. In comparison with results obtained by changes-in changes, results from QDD design on the left hand side panel, shows no effects, while results on the right hand side panel, shows positive impacts on the lower tail and no effects on the rest of the distribution.

3.5 Alternative estimation strategy

3.5.1 Individual-level affiliation results

Identification strategy Estimations of average treatment effects using the actual indicator of population that opted into the programme are presented to complement the intention-to-treat effects estimation. In this case, the definition of the treatment group is children living in households where the head of the household declared being enrolled on *Seguro Popular* and who were under 60 months of age on the first period; conversely, children from eligible households who reported not participating in the programme comprise the comparison group. The programme's treatment effect is estimated by comparing height-for-age of children in families who decided to enrol in the programme, to children born to families who were eligible but decided not to participate in the programme. Households that enrolled when the child was five or older belong to controls as they were considered beyond the critical age to benefit from the programme.

Descriptive statistics

Table 3.7 shows descriptive statistics for the children from households enrolled to SP and from households eligible but not enrolled. Children from enrolled households had lower height-for-age z-score. However, both groups had similar age and sex. The proportion of children from rural areas was double in controls; that population also had higher education; while treated population had larger household sizes; controls households were also richer and controls' parents were taller.

Table 3.7 Descriptive statistics of treated and controls (MxFLS)

	Controls			Treated		
Variable	N	Mean	Median	N	Mean	Median
Height-for-age z-score	2,679	-0.46	-0.47	2,481	-0.77	-0.74
Age in moths	3,159	58.93	58	2,902	58.32	58
Sex (boys=1)	3,159	0.51	1	2,902	0.51	1
Mother's height	3,062	157.09	157.2	2,825	155.17	155.2
Father's height	2,578	168.73	169	2,283	166.63	166.3
Parents' education	3,027	8.27	9	2,835	6.31	6
Household size	3,159	5.97	5	2,902	7.41	7
Asset index	3,028	6.16	6.39	2,769	5.23	5.43
Rural area	3,159	0.72	1	2,902	0.39	0
Marginalisation index	3,159	-1.23	-1.5	2,901	-0.67	-0.71

Notes. This table shows descriptive statistics of MxFLS data. The treated population are children from households enrolled to *Seguro Popular*.

Descriptives for children on the ENSANUT dataset are shown in Table 3.8. Treated children have lower average height and are older than controls. However, the design offsets the baseline level of height-for-age and the age effect. Samples are balanced between sexes, parents' height and education are similar across groups; household size is slightly greater for the treated; income decile is higher for controls; treated are more likely to live in rural areas; while controls were less likely to live in marginalised areas.

Using actual enrolment status as the definition of treated population, Figure 3.6 shows the evolution of height-for-age z-scores by age for different percentiles. In the figure, treated refers to children up to 9 years old from households that reported being affiliated, while controls refer to all children of 0 to 9 years old from eligible

Table 3.8 Descriptive statistics of treated and controls (ENSANUT)

	Controls			Treated		
Variable	N	Mean	Median	N	Mean	Median
Height-for-age z-score	25,353	-0.56	-0.56	10,659	-0.75	-0.75
Age in moths	25,353	55.43	54	10,659	64.31	68
Sex (boys=1)	25,353	0.5	1	10,659	0.5	1
Parents' height	19,291	157.89	157.15	8,832	156.68	156.05
Parents' education	21,061	2.2	2	9,448	2.03	2
Household size	25,353	5.19	5	10,659	5.44	5
Income decile	25,353	4.62	4	10,659	4.05	4
Rural area	25,353	0.52	1	10,659	0.69	1
Marginalisation index	25,353	-0.96	-1.26	10,659	-0.47	-0.59

Notes. This table shows descriptive statistics of ENSANUT data. The treated population are children from households enrolled to *Seguro Popular*.

households who reported not being enrolled. On the upper panel, results of the MxFLS survey show than on average, children at 0 years old start with similar z-scores, then controls remain taller for a few years, until eventually the treated group catches up with the controls, and by age 9 the treated have same height for age scores. This is also true for ENSANUT, treated catch up and end up with same height as controls. From these simple averages, we may hypothesise that the programme could have a long-term effect on childhood as treated height-for-age increases over time relatively more than controls.

Programme impacts

The first estimation of treatment effects follows a simple difference-in-differences approach. *Seguro Popular* treatment impacts on nutrition of children from households that declared being enrolled are compared to impacts on children from eligible households (without social security) that declared not being enrolled to the programme.

Table 3.9 presents difference-in-differences estimations of *Seguro Popular* mean effects on childhood height-for-age using data from MxFLS. There were no effects of the programme found on children's height-for-age z-scores. Interestingly, differences in height that were explained most importantly by income level and parents' height and less importantly by household size and rural residence, are no longer significant when adding fixed effects; marginalisation index and age explain all the variation in height-for-age z-scores. Table 3.10 shows the same results using the ENSANUT dataset. Positive treatment effects are strong, however, not clinically significant as the change is below 0.1 standard deviations. In this case, important factors to explain differences in height were genetics, age, household size and income; while sex

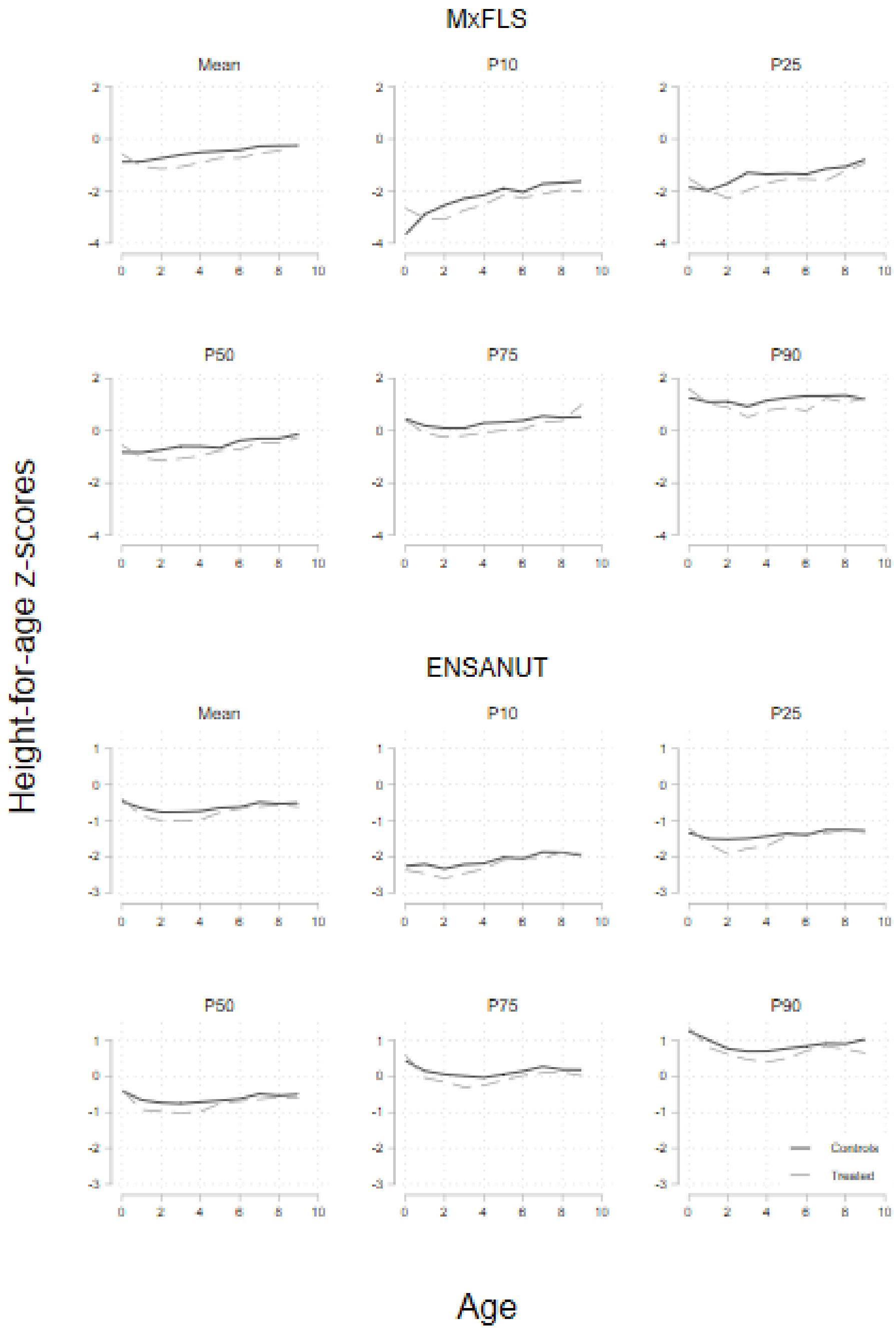

Fig. 3.6 Percentiles of height-for-age z-scores

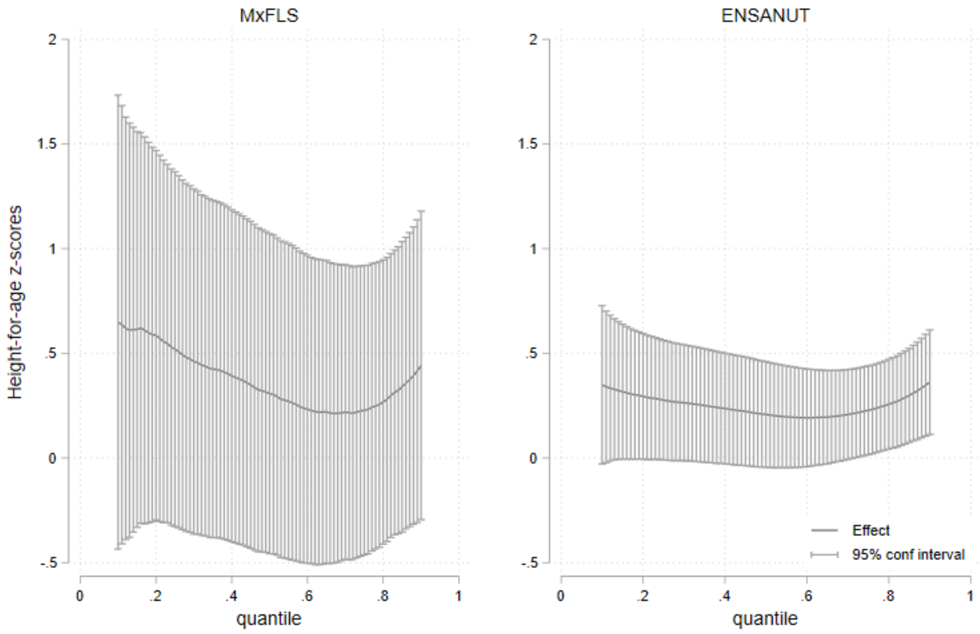

Fig. 3.7 Quantile treatment effects and 95% confidence intervals

and education were not relevant. Rural residence and marginalisation index were dropped due to collinearity, as they became redundant when including municipality fixed effects.

The quantile treatment effects in Figure 3.7, show a slight impact on the upper tail of the distribution of less than 0.5 standard deviation on the right panel and no-effects on the left panel. Table 3.11 confirms this with Kolmogorov-Smirnov and the Cramer-von-Mises-Smirnov statistics. There is evidence to suggest that the effects are different from zero and there is stochastic dominance as shown in Figure 3.8, right hand side panel. With higher effects on the right tail of the distribution, results suggest an increase in health pure inequality. These results may imply that the roll-out of *Seguro Popular* increased health inequalities by improving the health of children with a better nutritional health outcomes at baseline to a greater extent.

Changes in inequality

Results from the section above are confirmed in Table 3.12. Consistent with the main results obtained with the municipality-level affiliation indicator, there was a reduction in inequality found when using MxFLS data. Moreover, with ENSANUT data, results are consistent with those from the previous section and changes in the Gini coefficients of the treated and counterfactual distribution at the post-treatment period appear to indicate an increase in inequality.

Table 3.9 Average treatment effects of SP on height-for-age scores (MxFLS1-3)

VARIABLES	(1)	(2)	(3)	(4)
Treatment effect	0.126	0.204**	0.212	0.129
	(0.0821)	(0.0998)	(0.157)	(0.162)
Age in months		-0.00150	-0.0954***	-0.101***
		(0.00201)	(0.0151)	(0.0164)
Sex (boys=1)		-0.0862	-0.440*	-0.435
		(0.0565)	(0.252)	(0.291)
Mother's height		0.0347***	-0.0238	-0.0272
		(0.00519)	(0.0272)	(0.0281)
Father's height		0.0348***	0.0301*	0.0397*
		(0.00448)	(0.0161)	(0.0227)
Parent's education		-0.0108		
		(0.0115)		
Household size		-0.0277**		
		(0.0111)		
Asset index		0.0986***	-0.0117	-0.00200
		(0.0315)	(0.0738)	(0.0658)
Rural area		-0.0942	0.140	-0.00862
		(0.0771)	(0.360)	(0.354)
Marginalisation index		-0.0189	-0.0908	0.829***
		(0.0478)	(0.484)	(0.258)
Individual FE			X	X
Year FE			X	X
Municipality FE			X	X
State x year				X
Constant	-0.606***	-12.01***	3.299	3.234
	(0.0748)	(1.069)	(5.733)	(6.118)
Observations	3,553	2,540	1,830	1,815
R-squared	0.021	0.144	0.740	0.774

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.10 ATET of SP on height-for-age z-scores (ENSANUT 2006-12)

VARIABLES	(1)	(2)	(3)	(4)
Average treatment effects	0.142***	0.0567*	0.0972***	0.0973***
	(0.0302)	(0.0343)	(0.0348)	(0.0350)
Age in moths		-0.00142**	-0.00146**	-0.00148***
		(0.000569)	(0.000570)	(0.000570)
Sex (boys=1)		-0.0126	-0.0132	-0.0127
		(0.0176)	(0.0175)	(0.0176)
Parents' height		0.0275***	0.0217***	0.0216***
		(0.00118)	(0.00115)	(0.00115)
Parents schooling		0.0297	0.0104	0.00804
		(0.0231)	(0.0228)	(0.0227)
Household size		-0.0554***	-0.0409***	-0.0404***
		(0.00598)	(0.00596)	(0.00596)
Income decile		0.0414***	0.0550***	0.0584***
		(0.00547)	(0.00600)	(0.00609)
Rural area		0.0365		
		(0.0259)		
Marginalisation index		-0.182***		
		(0.0154)		
Year FE		x	x	x
Municipality FE			x	x
State x year trends				x
Constant	-0.709***	-5.096***	-4.161***	-4.165***
	(0.0190)	(0.187)	(0.184)	(0.185)
Observations	27,926	21,555	21,551	21,551
R-squared	0.011	0.132	0.228	0.230

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.11 Statistical tests of treatment effects and stochastic dominance

Null-hypothesis	MxFLS		ENSANUT	
	KS-statistic	CMS-statistic	KS-statistic	CMS-statistic
No effect: QTE(tau)=0 for all taus	0.36	0.26	0.0	0.01
Constant effect: QTE(tau)=QTE(0.5) for all taus	0.61	0.43	0.3	0.4
Stochastic dominance: QTE(tau)>0 for all taus	1	1	1	1
Stochastic dominance: QTE(tau)<0 for all taus	0.15	0.1	0.0	0.01

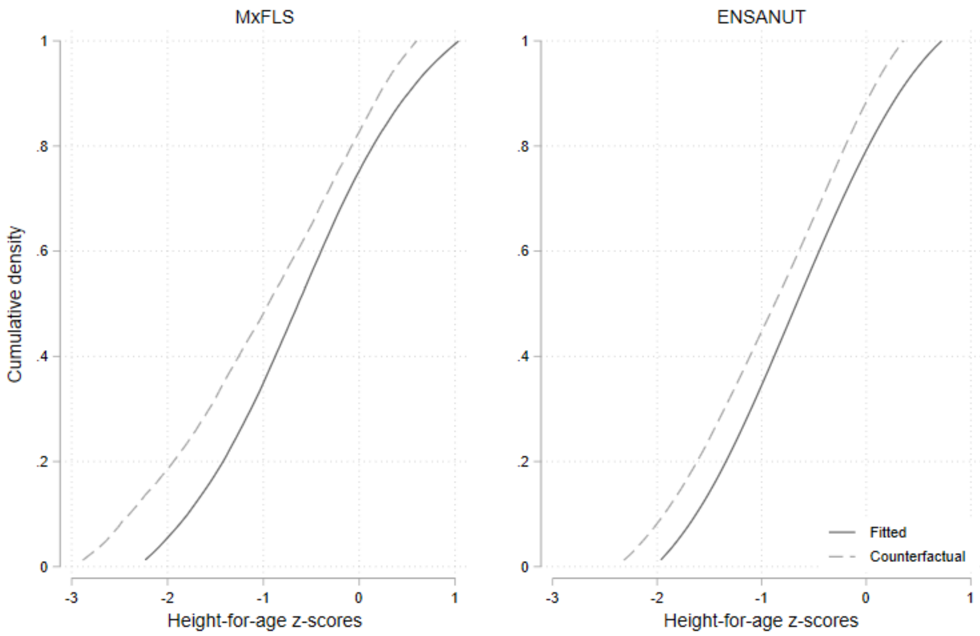

Fig. 3.8 CIC estimated and counterfactual distributions

Table 3.12 Generalised Gini coefficients of CIC distributions

		MxFLS		
Distribution	N	Index value	Std error	p-value
Counterfactual	81	0.54059659	0.00523442	0.0000
Estimated	81	0.47755768	0.00508294	0.0000
Test for stat. significant differences H0: diff=0				
F-stat	p-value			
74.647595	0.0000			
Test for stat. significant differences H0: diff=0				
Diff	Std. error	z-stat	p-value	
-.06303891	.00729627	-8.64	0.0000	
		ENSANUT		
Distribution	N	Index value	Std error	p-value
Counterfactual	81	0.46151452	0.00352156	0.0000
Estimated	81	0.62864396	0.00489626	0.0000
Test for stat. significant differences H0: diff=0				
F-stat	p-value			
767.90211	0.0000			
Test for stat. significant differences H0: diff=0				
Diff	Std. error	z-stat	p-value	
0.16712944	0.00603115	27.71	0.0000	

Table 3.12 shows the change in Gini coefficients and tests of significant differences. On the upper panel, for MxFLS, the Gini coefficient of the counterfactual distribution is slightly higher than the Gini of the estimated distribution. The difference in the Gini coefficients is 0.06 and statistically significant, implying there is a marginal decline in inequality. In the lower panel, the Gini coefficient of the counterfactual distribution obtained by the CIC model was smaller than the Gini coefficient of the estimated distribution. This implies an increase in inequality as the policy has a greater effect on the upper tail comparatively. The greater treatment effect on the children with the better health status increase the health pure inequality.

Alternative estimators

This section presents an alternative non-linear DiD estimator as a sensitivity analysis of our choice of model. Quantile DiD is similar to CIC as they use the change in the comparison group health distribution to construct a counterfactual distribution of treated children post-programme in the absence of treatment. However, their assumptions differ. QDiD assumes that the change in population shares around a given level of health is the same in the comparison group and in the treatment group in the absence of treatment, but CIC relaxes that assumption.

Figure 3.9 shows the quantile treatment effects of SP on the nutritional status of children using QDD estimator for both surveys. These results are not similar to those obtained by CIC. The alternative designs have positive effects on the lower tail of the height distribution, zero effect on the mid segment and negative effects on the higher part of the distribution.

3.6 Mechanisms

There are two paths through which healthcare interventions can have an impact on childhood health-for-age z-scores: by improving nutrition directly through programmes providing supplements; or by providing immunisation, hydration solutions, treatment of infectious diseases or any health issues that hinder the adequate absorption of nutrients and affect nutrition and growth (WHO, 1995). *Seguro Popular* did not have a specific nutrition intervention, as there were other social programmes like *Oportunidades* providing nutrition supplements to mothers and children. However, SP may have an impact on the nutritional status of children through improvements in their health status, specifically prevention and treatment of gastrointestinal or respiratory infectious diseases. According to the literature on childhood nutrition and health (Abramovsky et al., 2019; De Cao, 2015), infectious diseases as diarrhoea and respiratory diseases are the most prevalent conditions that have an impact on malnu-

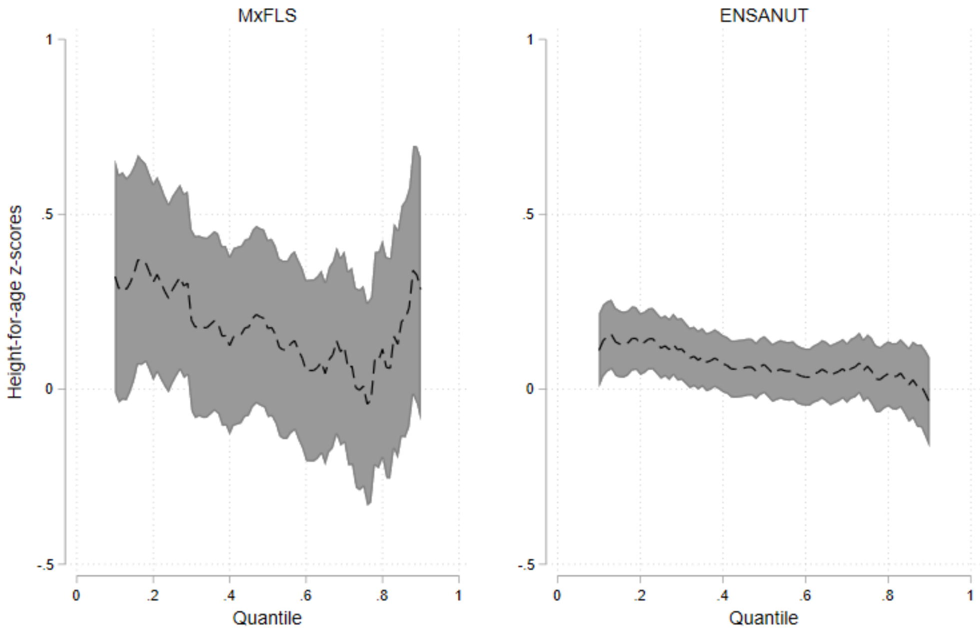

Fig. 3.9 QTE of alternative non-linear DiD estimators: QDD

trition, and stunting in particular. De Cao (2015) found that an episode of diarrhoea reduced the height of Filipino children by 2 to 3.5 cm. Based on these findings, an assessment of the impact of *Seguro Popular* on infectious diseases is proposed as a possible explanation of the channel through which the programme has an impact on childhood height-for-age. Equation 3.11 describes the difference-in-difference model to estimate the causal effect of SP on infectious disease:

$$y_{imt} = \alpha t_t + \gamma g_i + \tau SP_{imt} + x'_{imt}\beta + \pi_m + \delta_t + \theta_{st} + \varepsilon_{it} \tag{3.11}$$

where y_{imt} is a binary indicator of whether the child in household i, municipality m and year t had an episode of diarrhoea, influenza or parasites in the last four weeks. The estimation of the programme effect is the coefficient τ on the interaction between group and time $SP = g * t$. The treated population is defined as children from municipalities affiliated to SP when estimating intent-to-treat effects; when estimating average treatment effects on the treated, the treated population are children from households that reported being affiliated. The covariates x_{it} are the household and municipality characteristics plus child gender and age in months; α_t are the time-specific group-invariant effects; γ_i are the group-specific time invariant-effects; and ε_{it} is a random error term assumed to be uncorrelated with other regressors that captures all omitted factors.

Table 3.13 Intention-to-treat effects of SP on infectious diseases (MxFLS1-3)

	(1) Influenza	(2) Diarrhoea	(3) Parasites
Intention-to-treat	0.0666*	-0.0545**	0.00168
	(0.0365)	(0.0254)	(0.0170)
Age in months	-0.000117	-0.000442	3.28e-05
	(0.00178)	(0.00103)	(0.000986)
Sex (boys=1)	-0.0287	0.00737	0.000503
	(0.0608)	(0.0497)	(0.0362)
Asset index	-0.00544	-0.00677	-0.00582
	(0.0166)	(0.00913)	(0.00973)
Rural area	0.00372	-0.0978**	0.00148
	(0.0476)	(0.0423)	(0.0405)
Constant	0.302**	0.179**	0.0944
	(0.142)	(0.0763)	(0.0803)
Observations	6,135	6,135	6,074
R-squared	0.513	0.502	0.493

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.13 shows intention-to-treat effect of the programme on influenza, parasites and diarrhoea. Having the opportunity to enrol in *Seguro Popular*, regardless of affiliation status, reduced the likelihood of having a diarrhoea episode by 5.4 percentage points among children under 10 years of age. However, there is a marginally significant effect of increasing the likelihood of influenza by 6.6 percentage points. The impact of being affiliated to *Seguro Popular* on the same outcomes is shown in Table 3.14. The average treatment effects on the treated were marginal on parasites. The programme reduced the likelihood of having gastrointestinal parasites by 2.8 percentage points. Nevertheless, the effect is only marginally significant ($p<0.1$). These non conclusive effects reflect the also mixed effects of the programme found on childhood height for age. Results do not provide evidence of a clear causal path of the policy on height mediated by its effect on children infectious diseases.

3.7 Discussion

Malnourishment continues to be a public health issue in certain communities, and it is also an indicator of income inequality. Poor nutritional status is a serious health problem that causes inadequate physical and mental development on children with consequences that persist through adult age (Black et al., 2008). The period between

Table 3.14 Average treatment effects of SP on infectious diseases (MxFLS1-3)

	(1) Influenza	(2) Diarrhoea	(3) Parasites
ATET	-0.0227	-0.0290	-0.0286*
	(0.0298)	(0.0211)	(0.0162)
Age in months	0.000534	-0.000136	0.000284
	(0.00137)	(0.000905)	(0.000875)
Sex (boys=1)	-0.00106	0.0211	0.00243
	(0.0681)	(0.0531)	(0.0380)
Asset index	0.00384	-0.00736	-0.00500
	(0.0183)	(0.00959)	(0.00941)
Rural area	0.0106	-0.0696*	-0.00418
	(0.0483)	(0.0365)	(0.0407)
Marginalisation index	-0.104**	-0.0799**	-0.0183
	(0.0462)	(0.0343)	(0.0165)
Constant	0.0893	0.0677	0.0596
	(0.140)	(0.0818)	(0.0730)
Observations	5,296	5,296	5,242
R-squared	0.506	0.480	0.490

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

0-5 years of age is crucial in children nutrition (Frankenberg et al., 2005). During that time, nutritional status is highly sensitive to nutrition and health interventions. *Seguro Popular* does not have specific interventions focused on children's nutrition. However, it can have an indirect effect on nutrition by reducing out-of-pocket healthcare expenditures; or a direct effect by reducing the prevalence of infectious diseases.

Exploiting the exogenous variation in timing of the roll-out, this study assessed average and distributional impacts of *Seguro Popular* public insurance on childhood nutrition with data from two different surveys. Depending on the affiliation level data, two different specifications were used to find average impact: intention-to-treatment-effects were estimated using a municipality level affiliation; and average treatment effects for individual-level affiliation to *Seguro Popular*. Intention-to-treat effects were preferred as they had more credible assumptions.

Quantile treatment effects were estimated by two alternative methods: changes in changes and quantile difference-in-differences. The main difference between CIC and QDD approaches is the assumption in the counterfactual evolution. QDD assumes that a treated child in the τ^{th} quantile in the distribution before treatment will be at the same quantile after treatment in the absence of treatment. This restrictive assumption is relaxed in CIC. Results were consistent across methods; impact was greater on the lower end of the distribution, indicating a reduction on health inequality as individuals with lower height are better off.

In this study, timing is important in two ways; first to evaluate the impact of a childhood intervention, there is a critical period before 5 years of age; second, the programme has to be well established for a few years to accurately assess it. The greater impacts using ENSANUT, a more recent dataset, are compatible with the fact that SP impacts on health are only evident after the programme has been rolled out for a few years. Early evaluations suggested limited impacts on health outcomes (Barros et al., 2008; King et al., 2009, 2007), while recent studies that find clear health effects (Conti and Ginja, 2017; Pfutze, 2014). These results contribute to the debate on the benefits of expanding universal coverage in developing countries.

This study contributes to the literature of public health insurance, particularly related to childhood health and nutrition. While there are previous studies on childhood health, these are mainly focused on oncology outcomes (Ribeiro, 2013) or mortality (Celhay et al., 2019; Conti and Ginja, 2017; Pfutze, 2014). There is limited evidence of *Seguro Popular* on childhood nutrition. Only Turrini et al. (2016) investigate the impact of *Seguro Popular* on height-for-age z-scores. Using the Mexican Familiy Life Survey, with a different approach to the current study, they estimate intention-to-treat-effects of the programme. The authors, instead of following children through time, compare different children at different ages. In contrast, the present analysis takes advantage of the longitudinal structure and follows children through time. This approach compares treated and control children at a young age, before the programme has had an impact; and at an older age after children have received most to the benefit according to the biology of growth.

Another contribution is the examination of heterogeneous impact according to different outcome levels through quantile treatment effects. While most of the policy evaluation literature is concerned with average treatment effects, this study estimates the distributional impact of *Seguro Popular*. Averages do not provide information whether different groups receive different benefits. From this study perspective, it is more desirable that a public health policy raises the lower tail of the distribution – the individuals with greater need, rather than the median or the upper tail. The only study that estimates distributional impacts explores supply-side outcomes (Huffman and van Gameren, 2019). Moreover, the present study compares approaches for estimating quantile treatment effects varying the assumption to construct counterfactual distributions. These approaches examine the whole health distribution and allow to measure the policy impact on individuals with different health status.

There are certain limitations in this study. The sample size obtained from the panel Mexican Family Life Survey is limited, which makes it difficult to draw conclusions as large standard errors cause lack of precision in estimations. The sample size of the cross sectional *Encuesta Nacional de Salud y Nutrición* does not have that problem. However, impacts of *Seguro Popular* estimated with ENSANUT data should be taken with caution as the households surveyed are different across years. Each of the datasets used on this analysis has advantages. While both provide rich survey data, the panel configuration of MxFLS allows to follow individuals through three waves over 7 to 10 years, although with a comparative smaller sample size. ENSANUT is a repeated cross section that, accounting only for the two waves used in this study, has roughly five times the sample size of MxFLS. Therefore, confidence intervals are substantially narrower. The larger sample size allows for more precision in the estimation of SP impact effects.

Another limitation related to data availability regarding the roll-out of *Seguro Popular*. There is no information of the exact moment of affiliation to the programme in either survey. Given that early health interventions in children are mediators of health outcomes later in life, such as height for age, the timing of a health intervention such as SP is critical. Without information of the precise time in affiliation, it is not possible to know whether a child was exposed to the programme during a specific window period. The issue is partially solved taking the municipality-level roll-out indicator by assuming that the programme is present in a municipality after 10 families are affiliated (Bosch and Campos-Vazquez, 2014; Conti and Ginja, 2020;

Del Valle, 2014). In that case, it is clear whether the programme was available during a child specific age. The limitation of the approach is that the *Seguro Popular* was not randomly rolled-out, but based on arbitrary factors. The consequence is that, if areas with greater need were given priority, then estimates would be potentially downward biased. Therefore, SP impacts would be underestimated.

Overall, these results suggest an increase in height-for-age associated with the affiliation in *Seguro Popular*; moreover, the benefits were greatest for children in the lower tail of the nutrition distribution which are those with worse health status. These findings suggest an decrease in the pure inequality in childhood nutrition. The policy implication is the usefulness of non-linear DD methods for programme evaluation in the presence of heterogeneous effects. These results highlight the importance of taking into account that the effects of health policies may vary systematically along outcomes distributions.

Chapter 4

Effect of *Seguro Popular* on out-of-pocket payments, poverty and income distribution

Abstract

The purpose of this chapter is to assess the financial protection mechanism of *Seguro Popular*. Using a repeated cross-sectional dataset on households' income and expenditure, this study estimates the programme's impact on out-of-pocket healthcare payments, catastrophic payments, impoverishment and its anti-poverty and distributional effects. Average treatment impacts are estimated by fixed effects and instrumental variables models; while distributional effects are estimated using non-linear difference-in-differences methods and quantile regressions on instrumental variables. Results show a decrease in the amount of out-pocket-payments, but no effect on the incidence of catastrophic payments or on impoverishment due to out-of-pocket healthcare payments. There were no important effects on the overall income distribution derived from participation in *Seguro Popular*.

4.1 Introduction

By 2000, over half of the population in Mexico did not have health coverage. The health system was subdivided into three different subsystems: social security institutions covered the formally employed and their families financed by contributions from workers, employers and the federal government; government services provided by the Ministry of Health and financed by general taxes and user co-payments according to socio-economic status; and a private sector almost entirely financed by out-of-pocket payments.

The proportion of the population that declared not having access to social insurance in 2000 was 58.9% (57 million); while the proportion with social protection was only 39.6% (38 million) and 2.7% reported having private health insurance (Valdespino et al., 2003). The population with no social protection needed to pay out-of-pocket for healthcare any time they received services. Paying for healthcare out-of-pocket can have serious negative effects on welfare. Out-of-pocket (OOP) healthcare expenditure of 30% of available income or higher is considered catastrophic. In the extreme, a large enough OOP expenditure caused by a negative health event may push a household into poverty. Vulnerable and uninsured households face the serious risk of OOP healthcare expenditures driving them into a poverty trap restricting access to an adequate quantity and quality of healthcare (O'Donnell et al., 2007).

Lack of financial protection is a major barrier to health care in settings where out-of-pocket expenditures constitute the main source of health care financing. According to the World Health Organisation (WHO, 2010) out-of-pocket payments are the most inefficient and unfair method of financing healthcare as it violates the principle of financial justice which states that individuals should contribute to their health financing according to ability to pay and receive services according to health need (Jaimes and Flamand, 2016). *Seguro Popular* was the government's strategy to respond to this situation.

In 2003, the *Sistema de Protección Social en Salud* (System of Social Protection in Health) was created as a financial mechanism to protect the poor and uninsured population from the financial burden of out-of-pocket healthcare expenditures. The paramount aim of *Seguro Popular* was to expand financial coverage to the 50 million people that had no social security by reducing out-of-pocket healthcare expenditures and completely eliminate them for the poorest 20% of the population. The programme started with a basic set of 91 health interventions and 142 medicines from the Universal Health Services Catalog (Catálogo Universal de Servicios de Salud, CAUSES) that covered over 90 percent of the disease burden in Mexico (Popular, 2007). The catalog was complemented with the Fund for Protection Against Catastrophic Expenditures (Fondo de Protección contra Gastos Catastróficos or FPGC) that covered costly interventions which may result in catastrophic consequences for households affected. By 2019, the CAUSES catalog covered 294 interventions and 633 medicines and other healthcare inputs associated to them, including 65 interventions covered by FGPC[1].

This study investigates if the drop in out-of-pocket healthcare payments was caused by the introduction of *Seguro Popular*; and if the financial protection mechanism for uninsured households had an effect on the incidence and intensity of catastrophic and impoverishing expenditures; and their consequences on poverty levels and income inequality on the population. *Seguro Popular* can have an antipoverty effect through a reallocation of resources in the household. A reduction in OOP would increase available income for households to use for additional savings or consumption. Moreover, *Seguro Popular* can improve the productivity of households by reallocating human resources to labour. The additional healthcare coverage available to workers in the informal sector of the economy incentivise labour which will increase available income in the household. Evidence suggests that individuals already working in the

[1] Comisión Nacional de Protección Social en Salud. https://www.gob.mx/salud/seguropopular/documentos/catalogo-universal-de-servicios-de-salud-causes-2018-153111

formal sector do not switch to the informal economy, probably due to the additional benefits and better quality of social security for salaried workers (Barros et al., 2008; Conti et al., 2018). This additional income might have a larger effect on poorer population. The programme, by having a higher effect on the lower tail of the income distribution, might reduce income inequality in the population.

Using 10 waves from the *Encuesta Nacional de Ingreso y Gasto de los Hogares* (National Survey on Households Income and Expenditure or ENIGH), this study presents the impact of *Seguro Popular* on out-of-pocket healthcare expenditures, catastrophic expenditures, impoverishment, welfare and inequality. Policy impacts are estimated by a model of instrumental variables to find local treatment effects; a linear difference-in-differences design to find the programme average treatment effects; a non-linear difference-in-differences approach; and, a quantile instrumental variable model to estimate the policy's distributional effects.

4.2 Previous studies

There are several studies on the financial protection mechanism of *Seguro Popular*. The majority analyse its effect on out-of-pocket and catastrophic payments, most of them finding a positive effect of the programme on financial protection. Nevertheless, most studies are conducted on cross-sectional data and, therefore, unable to completely correct the endogeneity of the voluntary decision to enrol. The present study examines data from 2000 to 2018 and addresses the endogeneity using linear and non-linear fixed effects and instrumental variable models.

Knaul et al. (2005) predicted that with complete affiliation, out-of-pocket expenditures would drop 40% and catastrophic expenditures would decrease by half. However, these are results from simulated data assuming full affiliation. Gakidou et al. (2006) found that catastrophic expenditures for *Seguro Popular* affiliates are lower than for uninsured people; while Scott (2006) shows that the incidence of catastrophic health expenditures is lower across deciles for SP beneficiaries than for the rest of uninsured. However, these are descriptive studies and, therefore, only present associations and do not find causal relationships.

Intention-to-treat estimates by King et al. (2009) indicated a 23% reduction in catastrophic expenditures from baseline and a causal effect of 915 pesos on the complier group, but did not find any impact on medication spending. Despite its design being robust against political factors that could influence outcomes, results suffer from external validity issues according to the authors. Using the same original *Seguro Popular* evaluation data, Hernández-Torres et al. (2008), find that the likelihood of incurring catastrophic healthcare payments was 8% lower in affiliated households. This work was only conducted in two Mexican studies during the pilot phase.

Barros et al. (2008) found that SP led to substantial decrease in household health expenditures; and an increase of 4.2% in the amount of money beneficiaries spent in non-health consumption or savings. However, there was no redistributive impact. Moreover, the analysis was conducted during the early years after roll-out. Grogger et al. (2015), using the original experimental data from King et al. (2009) and similar data to the current study, found that SP has reduced out-of-pocket expenditures in rural areas and sharply reduced catastrophic spending among urban households with access to well-staffed health facilities.

Nikoloski and Mossialos (2018) using an instrumental variable model, analyse the effect of *Seguro Popular* and social security on out-of-pocket and catastrophic healthcare expenditures. They found no link on SP membership and OOP health spending, but a robust although small effect of SP on reducing catastrophic and impoverishing health expenditures. They attribute the lack of impact on OOP spending to a high and persistent spending on medications. However, the instrument used (municipality-level enrolment as a share of total potential membership) may have endogeneity issues as unobserved factors affecting both the individual decision to enrol and outcomes, could be also present in the instrument. Wirtz et al. (2012) offer some explanation for the high spending on medications even though these are covered by SP. They argue that the medications included in the package of interventions do not match the clinical needs of households, there are medication shortages in public facilities, and households continue to use private providers despite the fact of being affiliated to SP. Knaul et al. (2018) using propensity score matching and instrumental variables, found an impact of SP on reducing the likelihood of households to incur impoverishing expenditures but no effect on catastrophic spending. However, propensity score matching may not address endogeneity completely if unobservable factors correlated with membership status influence out-of-pocket healthcare expenditures. Moreover, they use the same instrumental variable as Nikoloski and Mossialos (2018) which may not satisfy the exogenous condition.

In a different context, Finkelstein and McKnight (2008) found that the establishment of Medicare 1965 in the United States had no impact on elderly mortality but was associated with a 40% decline in OOP healthcare spending for the top quartile of the OOP healthcare spending distribution. In the context of developing countries, Aji et al. (2013) study health insurance programmes in Indonesia using ordinary least squares, instrumental variables and fixed effect approaches and find that coverage of two insurance programmes had a negative statistically significant impact on OOP expenditures of between 11% and 50%. Wagstaff and van Doorslaer (2003) present and compare catastrophic and impoverishment approaches to measure fairness in healthcare OOP payments. They apply their methods using data from Vietnam in 1993-1998 and find a reduction in both incidence and intensity of catastrophic payments; moreover, the poverty impact of OOP payments both in incidence and intensity declined over time.

4.3 Methods

4.3.1 Data

The main dataset used is the National Survey of Household Income and Expenditure from Mexico (*Encuesta Nacional de Ingreso y Gasto de los Hogares*, ENIGH). It is a cross-sectional dataset conducted every two years with varying number of households surveyed. ENIGH provides data on the amount, source and distribution of households' income and expenditures. Additionally, the survey includes information about occupational and socio-demographic characteristics of household members; infrastructure characteristics of dwellings and household assets. For the 10 waves used for this study (the even years from 2000 to 2018) sample sizes increased each year from around 9,000 to 75,000 households surveyed per wave. The data is representative at the national and state levels. In addition to income and expenditure data, the survey also includes information about household access to public policies such as *Seguro Popular* and *Oportunidades*, a conditional cash transfer programme.

In addition to survey data, two administrative datasets were used for the analysis. The first is a dataset with the total number of people enrolled to *Seguro Popular* by municipality for each year. This information is used to construct an indicator for the municipality-level roll-out. A municipality is assumed to being enrolled after 10 families living in that municipality have affiliated (Bosch and Campos-Vazquez, 2014; Conti and Ginja, 2020; Del Valle, 2014). The second is an administrative dataset on a marginalisation index by that summarises development indicators on the municipality-level such as percentage of population with no access to education, services, or low income, that resides in small communities, among other indicators that may hinder development[2].

4.3.2 Identification strategy

This study investigates the effect of the expansion in coverage introduced by *Seguro Popular* on households' out-of-pocket healthcare payments and the financial protection effect of the programme on poverty levels and on the income distribution in the

[2] http://www.conapo.gob.mx/work/models/CONAPO/Resource/1755/1/images/01Capitulo.pdf

population. By simply comparing outcomes from enrolled to not enrolled but eligible population we would get biased results as treatment status is not randomly assigned. Instead, people voluntarily enrol into the programme. It is likely that individuals who decide to enrol would expect to have greater benefits than those who decide not to enrol. Therefore, treatment effects would have an upwards bias caused by positive selection on unobservables (Blundell and Costa-Dias, 2009).

To address the potential bias resulting from endogeneity (i.e. non random) in the decision to enrol, the strategy used is to instrument the endogenous treatment variable with the municipality level roll-out. Specifically, the instrument takes a value of one after more than 10 families are enrolled in a specific municipality in a year from 2002 to 2018 (Bosch and Campos-Vazquez, 2014; Conti and Ginja, 2020; Del Valle, 2014). To verify the results obtained with the instrumental variables model, a second identification strategy to account for the endogeneity in enrolment is to estimate intention-to-treat effects with a fixed effects model exploiting the geographical variation in the timing of *Seguro Popular*'s roll-out. Outcomes in municipalities where the programme was already rolled-out are compared to households from municipalities not yet enrolled, regardless of their participation status. Both strategies provide average impacts of the programme. Additionally, quantile treatment effects are estimated along the distribution of outcomes using changes-in-changes and instrumental variables quantile regression models.

4.3.3 Outcomes

Let w_i be defined as the total income of household i, and T_i its total out-of-pocket healthcare spending. If household i spends on healthcare 20% or more out-of-pocket of its monthly available income, then T_i is considered catastrophic. The indicator C_i is equal to one if a household exceeded the OOP healthcare catastrophic payment threshold and 0 otherwise. Such an indicator does not take into account if the household with catastrophic spending is 'rich' or 'poor'. Wagstaff and van Doorslaer (2003) propose a weighted measure that is sensitive to the relative income level of households. Poorer households will have relatively higher importance. Thus C_i is weighted by the households rank (from poorest to richest) in the income distribution as:

$$s_i = 2\frac{N+1-r_i}{N} \tag{4.1}$$

where r_i is the absolute ranking of household i and N is the population size. The weight, s_i for poorest will be 2 and for the richest will be $2/N$.

Catastrophic OOP payment can lead to impoverishment: a non-poor household may fall under the poverty line z as a result of OOP healthcare spending[3]. Household income is divided by number of members to get a per capita measure. The indicator p_i^{pre} equals 1 if household i's per capita income gross of OOP healthcare payments is below a poverty line PL and 0 otherwise. Similarly, p_i^{post} indicates that a household per capita income is below the poverty line after substracting OOP healthcare spending. A household had impoverishing OOP healthcare expenditures if its per capita income level is above the poverty line before payments $p_i^{pre} = 0$, but falls below the poverty line as a result of OOP healthcare payments $p_i^{post} = 1$. These poverty headcount indicators simply provide information on whether a household is poor but do not account for how far below the poverty line the household falls. In contrast, the poverty gap calculates the degree of poverty of a household as the distance between the poverty line and a household's per capita income $(PL - w_i)$. The poverty gap before OOP healthcare payments is defined as $g_i^{pre} = p_i^{pre}(PL - w_i)$. Similarly, we define $g_i^{post} = p_i^{post}(PL - (w_i - T_i))$ as the distance between the poverty line and the household per capita income after OOP healthcare payments.

In order to explore the financial protection impact of the programme, this study estimates the effect of the change in OOP healthcare expenditures both on the poverty headcount and the poverty gap – i.e. if the establishment of *Seguro Popular* reduces the likelihood of a household to fall below the poverty line; and also if there is a reduction in monetary terms on the distance between poverty line and the household income level as a result of OOP healthcare spending. First, to assess the effect on the poverty incidence, Δp_{imt} is defined as the change in poverty status due to healthcare payments as the difference between before and after OOP payment poverty headcounts $p_{imt}^{post} - p_{imt}^{pre}$ for household i. In other words, the indicator Δp_{imt} equals 1 if the household had impoverishing healthcare expenditures and 0 otherwise. Similarly, to estimate the impact of the programme on the poverty intensity, Δg_{imt} is defined as the difference in poverty gaps before and after health expenses $g_{imt}^1 - g_{imt}^0$. There are three cases depending on the household net and gross OOP health payments poverty status:

$$\Delta g_{imt} = \begin{cases} 0 \ , & \text{if } p_{imt}^{post} = p_{imt}^{pre} = 0 \\ T_i \ , & \text{if } p_{imt}^{post} = p_{imt}^{pre} = 1 \\ PL - (w_i - T_i) \ , & \text{if } p_{imt}^{post} = 1; p_{imt}^{pre} = 0 \end{cases} \tag{4.2}$$

[3]The standard poverty line for upper-middle income countries used here is defined by the World Bank at 5.50 USD a day per capita measured at 2018 prices and adjusted for PPP.

The first case in equation 4.2 refers to non-poor households, both their before and after healthcare payments income is zero, and therefore, their difference is 0 too; if household i is poor before and (necessarily) after health expenses, its difference in poverty gap will be T_i, precisely the household OOP health expenditures; and the last case, the most interesting in this context, is when a household is non-poor before health expenses, but falls below the poverty line because of OOP health expenditures. In this case, the difference in poverty gaps will be precisely the poverty gap after OOP healthcare payments.

To explore the impact of *Seguro Popular* on households' welfare, the share of food expenditure of households' budget is examined. This widely used welfare measure (Deaton, 1997) is based on the assumption that food expenditure share in budget identifies welfare for households with different demographic composition. Engel's Law[4] states that there is a negative relationship between food share and total income. Therefore, food share of budget is assumed to be an inverse measure of welfare.

4.3.4 Econometric analysis

Instrumental variables

To estimate the effect of *Seguro Popular* on the outcomes of interest, this analysis assumes a linear model where outcomes depend on households' affiliation status and on socio-demographic characteristics. The model is estimated as:

$$Y_i = \alpha + x_i'\beta + \delta D_i + \varepsilon_i \tag{4.3}$$

where x' is a vector of covariates and β a vector of associated coefficients; D_i is the indicator of programme enrolment of household i, and δ is the coefficient of the treatment effect of SP on the outcome Y_i; ε_i represents the unobservable individual heterogeneity assumed to be random, and uncorrelated with the regressors. The decision to affiliate, D_i, is endogenous. As households decide voluntarily to enrol in *Seguro Popular*, treatment status is correlated with the error term. This would introduce a bias in the estimates as affiliates are potentially different from households that decide not to participate. One way to address the selection bias is to instrument the endogenous affiliation status variable. In the first stage, the households' probability of enrolling is estimated as:

$$D_i^* = z_i'\gamma + v_i \tag{4.4}$$

[4]Engel, E., 1857. Die productions-und consumtionsverhältnisse des königreichs sachsen. Zeitschrift des Statistischen Bureaus des Königlich Sächsischen Ministeriums des Innern, 8, pp.1-54.

where D_i^* is the probability of household i to enroll in SP; z_i' represents the vector of instruments with associated coefficients γ, and v_i is the residual term. The instrument is a binary variable that equals one if the programme is present in a municipality. The instrument indicates if the programme is available for households to enrol, in that sense one would expect that the instruments will cause health expenditures to change only through enrolment status.

An instrument satisfies the exclusion restriction if it is not correlated with the error term conditional on the household and municipal covariates. In other words, if individual level enrolment status and municipal level enrolment are correlated to another factor that is also a determinant of households' out-of-pocket healthcare payments, the instrument would not be valid. An example of such a factor might be the availability of healthcare facilities in a municipality. Evidence suggests that, indeed, healthcare availability is one of the main influences for municipalities' adoption of *Seguro Popular* and therefore households' affiliation (Conti and Ginja, 2017). *Seguro Popular* was originally rolled-out in municipalities with the required infrastructure for effective implementation. The first municipalities to enrol were the ones that already had healthcare facilities; followed by the ones that had to invest in healthcare infrastructure to comply with the requirements to implement. The availability of hospitals and clinics in a municipality would drive local governments to introduce the programme and also for households to enrol; and at the same time, healthcare service availability influences families in their decisions to spend out-of-pocket on healthcare. These differences in healthcare availability across municipalities are accounted for with the inclusion of municipality's characteristics, healthcare infrastructure, marginalisation index, and rural or urban area of household location. These variables would measure the degree of development and therefore the availability of healthcare facilities. Moreover, the validity of the instruments is tested using endogeneity tests and the Cragg-Donald-Wald F statistic for weak instruments.

Fixed effects

To verify the robustness of the results to different specifications, this section presents a model to estimate intention-to-treat effects by exploiting the variation in the timing of SP's implementation at the municipality level. The roll-out began in 2002 with a pilot in 26 municipalities from 5 states and the official start was in 2004. Full implementation at the municipal level was 99% complete by 2007, while at the individual level full enrolment took roughly a decade. Even though the implementation was not random, the natural experiment created by the progressive roll-out is useful for evaluation purposes. The specification in this section is a flexible event-study approach that allows for heterogeneous treatment effects in time of exposure to treatment after roll-out, while looking for any trends in the outcomes in the pre-treatment periods. The treated population is defined as households located in municipalities where the programme is ongoing and the reference period are households situated in municipalities the year prior to implementation. The data is from every two years starting in 2000 through 2018 which means there are households from 7 years before treatment (2000 data on municipalities rolled-out in 2007) to 16 years after treatment (2018 data on municipalities rolled-out in 2002). The relevant parameter is the intention-to-treat effects (ITT) to address selection bias introduced by the individual's decision of whether or not to enrol on the programme. Outcomes are modelled using the following specification:

$$Y_{imt} = \mu_m + \delta_t + x'_{imt} + \sum_{j=-7}^{-2} \tau_j^0 SP_{mt} + \sum_{j=0}^{16} \tau_j^1 SP_{mt} + \varepsilon_{imt} \tag{4.5}$$

where SP_{mt}, $j = (-7, 16)$ are indicators that together with j represent the event of a municipality implementing *Seguro Popular*; τ_j is the effect of the programme being available on a municipality j years after roll-out. The dummy for the period at one year before treatment $j = -1$ is omitted and taken as the base year similarly to Conti and Ginja (2017) and Bailey and Goodman-Bacon (2015). The period $j = 0$ is assumed to be part of the period under treatment. The first set τ^0 captures all the differences if any by year before the programme, and the second set τ^1 are the differential effects by year under treatment. All effects are estimated with respect to $j = -1$. Thus, if any of the pre-treatment τ coefficients are statistically significant, we have a violation of the parallel trends assumption. The fixed effects terms are μ_m, a set of dummy variables to account for the permanent municipality-level differences; and δ_t is the effect of any time patterns common to all units. The vector covariates x_{imt} account for time-varying household and individual socio demographic characteristics; and the error term ε_{imt} that captures all the remaining omitted factors is assumed to be random and uncorrelated with other regressors.

Non-linear difference-in-differences

When the policy expansion may have heterogeneous treatment effects, the average treatment effect on the treated may miss important information along the outcomes distribution. While there may be heterogeneous impacts of the programme arising from individuals' differences in observed characteristics like income or residence strata; or heterogeneity according to unobserved expected gains from the policy (Heckman, 1979), this section explores heterogeneity in the effect according to outcome levels. To find differences in the impact of *Seguro Popular* across the distribution of out-of-pocket and impoverishing healthcare payments, the approach used is the changes-in-changes model, a non-linear difference-in-differences design introduced by Athey and Imbens (2006) and extended by Melly and Santangelo (2015) to allow for covariates. The method is a generalisation of the difference-in-difference approach that uses the change in the distribution of the control group over time to estimate the counterfactual, i.e. the distribution of the treatment group in the absence of treatment. The parameter estimated is the quantile treatment effect on the treated (QTT) defined as the difference of the two potential outcome distributions for the treatment group at any quantile $\tau \in (0, 1)$. In a randomly assigned programme, the QTT would be the difference at the τth quantile of the treated households' income distribution and the distribution in controls for a given year. However, in a non-experimental setting, the treatment effect would be the difference between the distribution of outcomes of the treated population in the post treatment period and the counterfactual distribution of outcomes constructed as the treated distribution had they not been treated.

A difference-in-differences two-period framework is used as the general identification strategy for estimating the quantile treatment effects of the programme. A household belongs to group $G = \{0, 1\}$ where group 1 is the treatment group and is observed in periods $\tau \in \{0, 1\}$. Where only households in group 1 on period 1 are affected by the policy. The treated population is defined as the households who declared being enrolled in the programme and the control group are households who were eligible for the programme but decided not to enrol. An advantage of the approach is that it allows for selection on unobservables as long as they do not vary across time within groups.

Because the dataset used is a repeated cross-section, there is no available information on individuals across time. Thus, instead of the two-period standard model, all years of the repeated cross-sectional are pooled in the analysis. The indicator $\tau = 0$ is defined as the outcomes before OOP healthcare payment, while $\tau = 1$ refers to the same year-observation but after OOP healthcare payments. This approach allows to give the effect of *Seguro Popular* on out-of-pocket payments a causal interpretation and allows for heterogeneity according to the income distribution. In particular, the goal is to investigate if the policy has a greater impact on lower tail of the distribution, the poorer households.

Following Athey and Imbens (2006) using potential outcomes notation literature (Rubin, 1974, 1978), the realised outcome is defined as,

$$Y = (1 - I) \cdot Y^N + I \cdot Y^I \tag{4.6}$$

Where Y^N denotes the outcome if a household is not enrolled in the programme, and Y^I if the household participates; and I is the interaction, $I = G * T$. There is a vector of covariates X whose distribution has support $\mathbb{X}$. The outcome of a household in the absence of treatment satisfies,

$$Y^N = h(X, T, U) \tag{4.7}$$

where U is the unobservable individual characteristics of Y and the production function $h(t, x, u)$ is increasing in u for $t \in \{0, 1\}$ and for all $x \in \mathbb{X}$. The identifying assumption of this model is that, in the absence of treatment, the distribution of unobservable characteristics U can vary across groups, but within groups is constant over time. This assumption is analogous to the parallel trends assumption in the linear DiD design. As each group has a different production technology, the expected benefit may vary across groups.

Let F_Y^I be the distribution of available income of treated households in period t; and F_Y^N, the distribution of control households at period t, where $t = 0$ means pre-payments period and $t = 1$, post-payments period. We can identify the conditional distribution Y_{11x}^N over the quantiles $0 < \tau < 1$ as:

$$F^{-1}_{Y^N|11_x}(\tau) = F^{-1}_{Y|01_x}\Big(F_{Y|00_x}\Big(F^{-1}_{Y|10_x}(\tau)\Big)\Big) \tag{4.8}$$

Intuitively, equation 4.8 says that a household at the τ quantile of the outcome distribution in period 0 and group 0 would be at the $F_{Y|00_x}\Big(F^{-1}_{Y|10_x}(\tau)\Big)$ quantile of the outcome distribution in the same period but in group 1. The conditional quantile treatment effect for group 1 in period 1 for the covariates x:

$$\Delta^{QE}(\cdot|x) = F^{-1}_{Y^I|11_x}(\cdot) - F^{-1}_{Y^N|01_x}(\cdot) \tag{4.9}$$

$$\Delta^{QE}(\cdot|x) = F^{-1}_{Y^I|11_x}(\cdot) - F^{-1}_{Y|01_x}\Big(F_{Y|00_x}\Big(F^{-1}_{Y|10_x}(\tau)\Big)\Big) \tag{4.10}$$

However, given the difficulty to translate the conditional treatment effects in the context public policy, unconditional treatment effects are more relevant. The unconditional distribution of outcomes for group 1 in period 1 is obtained by integrating the conditional outcome distribution over the distribution of covariates:

$$F_{Y^I|11}(y) = F_{Y|11}(y) = \int_{\mathbb{X}} F_{Y|00_x}(y) dF_{X|11}(x) \tag{4.11}$$

$$= \int_{\mathbb{X}} F_{Y|10_x}\left(F^{-1}_{Y|00_x}\Big(F^{-1}_{Y|01_x}\big(y\big)\Big)\right) dF_{X|11}(x) \tag{4.12}$$

The unconditional treatment effects on the potential outcome are given by:

$$\Delta^{QE}(\cdot) = F^{-1}_{Y^I|11}(\cdot) - F^{-1}_{Y^N|11}(\cdot) \tag{4.13}$$

Figure 4.1 illustrates the estimation of a counterfactual distribution: for any quantile, we find the level of outcome y for the distribution of the treated before treatment; then, we find the quantile associated to that level of y on the control group before treatment distribution, and the outcome level y' on the controls after treatment for that quantile.

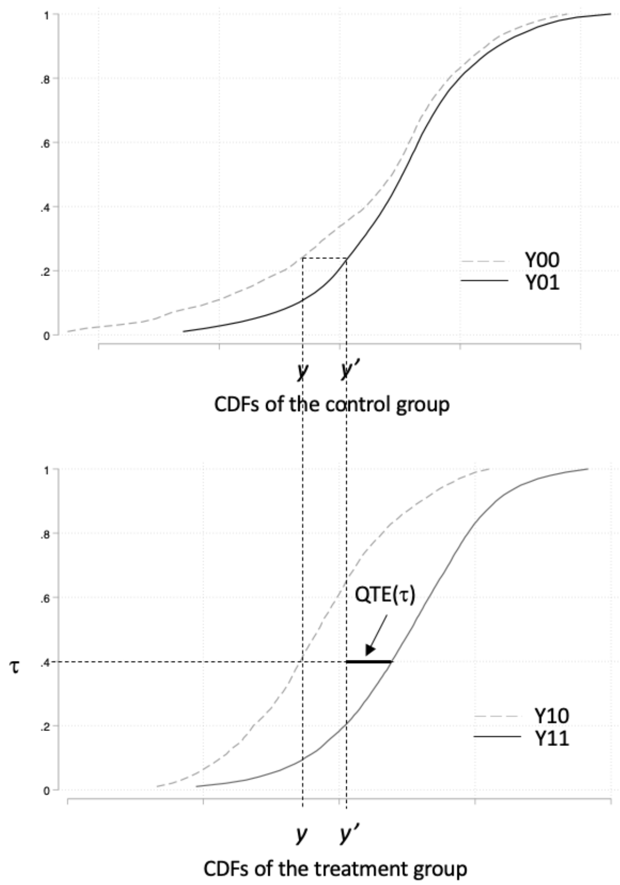

Fig. 4.1 Changes-in-changes distribution transformations

Quantile local average treatment effects

This section presents the methods used to estimate the quantile treatment effects of *Seguro Popular* on welfare. The measure of welfare used here is the food expenditure share of total household expenditure. There is a negative relationship between food share and total expenditure. This means that holding everything else constant, the households with the smaller income will have a larger food share. Distributional impacts of the programme are particularly informative as average effects can have ambiguous interpretations. A positive average effect on the food expenditure share could be an increase or decrease in overall well-being depending on which part of the distribution it has the most impact. An increase in food share in the lower tail of the distribution could mean a positive effect on welfare if households are spending more on food resources that would have been used on healthcare in the absence *Seguro Popular*. It is of particular interest in this study to find the effect of *Seguro Popular* on lower income households, being the policy's main target population.

To analyse the distributional impact of *Seguro Popular* on welfare, quantile treatment effects are estimated using a non-parametric instrumental variables model proposed by Frölich and Melly (2013). The model considers the effect of a binary treatment variable D on a continuous outcome variable Y. Let Y_i^1 and Y_i^0 be the potential outcomes of individual i where Y_i^1 would be observed if individual i receives the treatment, and Y_i^0 would be observed otherwise. The observed outcome Y_i is defined as $Y_i \equiv Y_i^1 D_i + Y_i^0(1-D_i)$. The quantile treatment effect of the policy (QTE) represents the distributional impact and is defined as:

$$\Delta^\tau = Q^\tau_{Y^1} - Q^\tau_{Y^0} \tag{4.14}$$

where $Q^\tau_{Y^1}$ is the τ quantile of Y^d. The treatment variable D is endogenous and identification is by a binary instrumental variable, Z. In this case, the municipality level indicator of affiliation to *Seguro Popular*. Thus, the QTE is identified under the assumptions that there is a complier population who respond in the intended way to Z; that no individuals who defy their treatment status; that common support of X exists for Z=0 and Z=1, and that the instrument satisfies the exclusion restriction and is conditionally independent. The quantile processes for $\tau\epsilon(0,1)$ are identified and estimated separately. The distribution function of Y^1 for compliers:

$$\frac{E[1(Y \leq u)D|Z=1] - E[1(Y \leq u)D|Z=0}{E[D|Z=1] - E[D|Z=0]} \tag{4.15}$$

This unconditional distribution function is inverted to obtain the unconditional quantile function. Conditional on X:

$$F_{Y^1|_c}(u) = \frac{\int (E[1(Y \leq u)D|\mathbf{X}, Z=1] - E[1(Y \leq u)D|\mathbf{X}, Z=0])\, dF_x}{\int (E[D|\mathbf{X}, Z=1] - E[D|\mathbf{X}, Z=0])\, dF_x} \tag{4.16}$$

$$= \frac{E[1(Y < u)DW]}{E[DW]} \tag{4.17}$$

where

$$W = \frac{Z - \Pr(Z=1|X)}{\Pr(Z=1|X)\{1-\Pr(Z=1|X)\}}(2D-1) \tag{4.18}$$

The distribution of Y^0 for the compliers is identified analogously replacing D with 1-D. Thus, the QTE is identified as the difference between the quantiles:

$$\Delta_c^\tau = F^{-1}_{Y^1|_c}(\tau) - F^{-1}_{Y^0|_c}(\tau) \tag{4.19}$$

4.4 Results

4.4.1 Descriptive analysis

Descriptive statistics

Table 4.1 summarises some characteristics of the sample. There are over 300,000 households in the sample combining ten waves over 18 years[5]. Household monthly total income was 14,676 Mexican pesos (MXN) in constant terms from 2018 (≈US$ 732); household expenditure was 11,361 MXN (≈568 US$), monthly average household food expenditure was 3,295 MXN (≈165 USD). Households had 3.82 members in average; 28% of whom were over 65 years olds and 42% were children under 5. Monthly average OOP healthcare payments were 302 MXN (≈15 US$), which represent 2% of total income. Thirteen percent of the sample had catastrophic expenditures of at least 20% of their income. Before OOP healthcare payments, 15% of the sample were below the poverty line, and 16% after payments. The poverty gap before OOP payments was 68 MXN (≈3.4 USD) and after OOP payments, the poverty gap was 79 MXN (≈4). Taking the mean of all waves with information on affiliation (2004-2018), 44% of households had at least one member enrolled in *Seguro Popular*. Heads of household were 75% male, 48.5 years old in average, 73% of them had completed primary school, 27% also high school. About 70% of households lived in a low marginalised area, while 10% lived in a highly marginalised area; 36% of the sample lived in a rural location.

Graphical analysis of OOP healthcare payments

This section explores out-of-pocket healthcare expenditures. Figure 4.2 shows OOP payments for health care as a percentage of total household expenditure for years 2000 to 2018. On average, OOP payments account for between 2 and 3 percent of

[5] 2000, 2002, 2004, 2006, 2008, 2010, 2012, 2014, 2016 and 2018

Table 4.1 Descriptive statistics, 2000-2018

	N	Mean	Std. Dev.	Min	Max
Household total income (MXN)	299,318	14,676.79	35,077.45	0	13,557,326
Household total expenditure (MXN)	303,202	11,631.53	14,279.19	0	2,874,399
Household size	301,267	3.82	1.94	1	43
OOP healthcare payments (MXN)	300,973	302.77	1,749.74	0	327,032
OOP share of total income	300,972	0.02	0.05	0	1
Catastrophic payments ($\geq$ 20%)	303,202	0.13	0.33	0	1
Headcount poverty before OOPHE	303,202	0.15	0.36	0	1
Headcount poverty after OOPHE	303,202	0.16	0.37	0	1
Poverty gap (MXN) before OOPHE	299,318	67.96	198.94	0	1,491.15
Poverty gap (MXN) after OOPHE	299,038	79.61	607.16	0	279,324.40
Food expenditure (MXN)	300,973	3,295.92	2,701.08	0	600,611.40
HH enrolled in *Seguro Popular*	274,005	0.44	0.50	0	1
Head of household's sex	300,628	0.75	0.43	0	1
Head of household's age	300,628	48.51	15.87	12	110
HH completed primary school	300,628	0.73	0.44	0	1
HH completed high school	300,628	0.27	0.44	0	1
Number of 65+ year olds in HH	301,267	0.28	0.58	0	5
Number of 5- year olds in HH	301,267	0.42	0.70	0	9
Rural area	301,253	0.36	0.48	0	1
Marginalisation index, 2010		%			
Very low	300,965	53.09			
Low	300,965	16.94			
Medium	300,965	19.69			
High	300,965	4.57			
Very high	300,965	5.03			

Note: 2018 Mexican pesos, 1 USD=20 MXN.

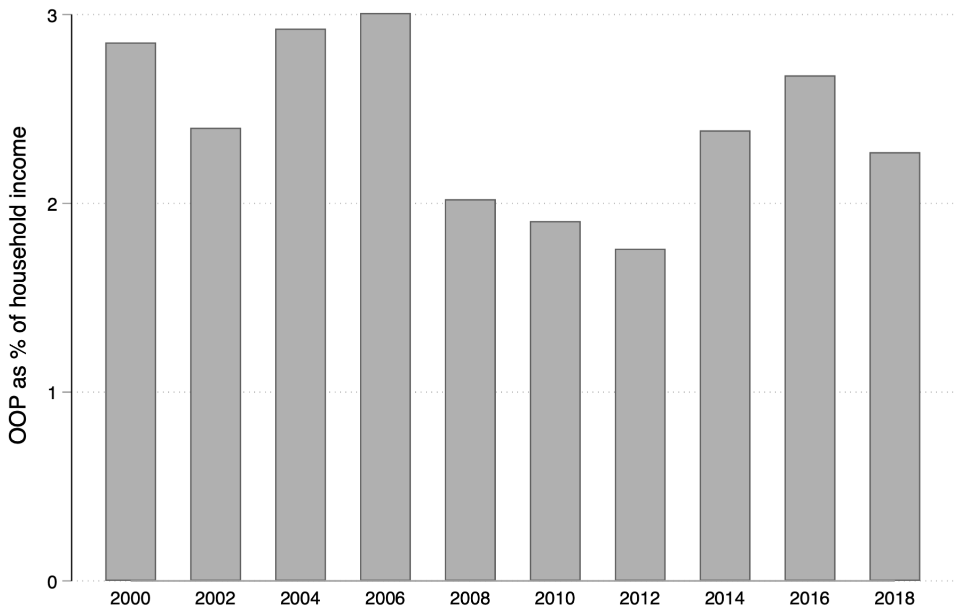

Fig. 4.2 OOP Payments as a Percentage of Total HH Income-Average by year, Mexico, 2000-2018

household expenditure in the nine years in the sample. The share of OOP payments do not show a trend over time but the average appears to decrease after 2006. This could be an effect of *Seguro Popular* coverage if the decline in OOP healthcare payments are attributable to the reform.

Figure 4.3 explores the progressivity of health payments by examining their share of total income when income varies. Out-of-pocket payments for health care are displayed as a percentage of household income by quintile groups of equivalent income for years 2000 to 2018. Out-of-pocket payments show a negative relationship with income as they seem to decrease their share when income rises. This indicates some degree of regressivity in OOP payments. A health system where OOP payments represent a large source of health financing is expected to be regressive as poorer people would necessarily use a larger share of income in healthcare.

Figure 4.4 explores income inequality descriptively between affiliates and non-affiliates. The Lorenz curves compare the total income distribution after OOP payments for households enrolled in the programme with eligible households that are not enrolled for the nine waves. The Lorenz curve of the treated dominates the curve for controls, implying that the total income distribution after OOP payments is more equal for treated households. This may be attributable to the programme if poor people among beneficiaries are spending a smaller proportion of their income on healthcare than richer people due to the coverage of *Seguro Popular*. Coverage

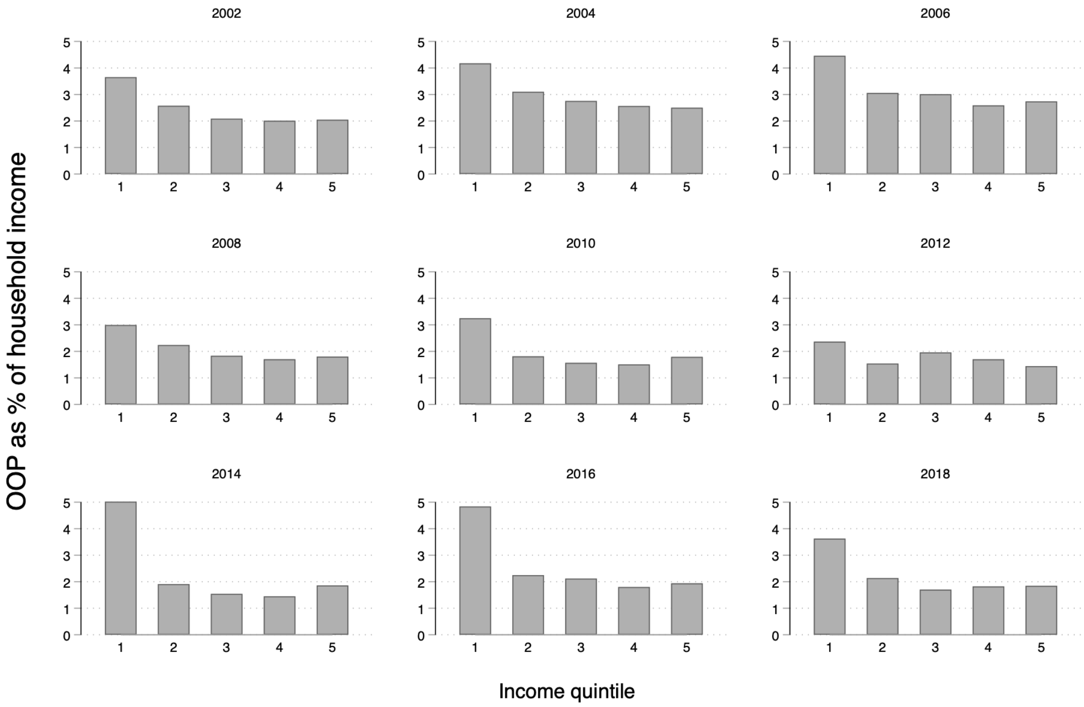

Fig. 4.3 OOP Healthcare Payments as a Percentage of Total HH Income - Income Quintile

against financial risk means that money that would have been spent on healthcare in the absence of *Seguro Popular* is now additional income for the household. If SP is effectively targeting poorer population, then there is more additional income on the left of the income distribution, reducing inequality.

Figure 4.5, presents a summary on poverty dominance between treated and controls. The device known as the three 'I's of poverty (TIP) curves, provides a summary on incidence, intensity and inequality dimensions (Jenkins and Lambert, 1997). The figure shows the cumulative poverty gaps of households ranked by income from poorest to richest. The curve for the treated dominates the curve of controls. Hence, the poverty after out-of-pocket healthcare payments of the treated is higher than the control households in a pooled sample of nine years. The incidence is measured in the horizontal axis as the length of the non-flat section. In Fig 4.5, the non-flat section of the treated curve goes beyond the 0.3 in the horizontal axis, implying a poverty incidence over 30%; for controls, the poverty incidence is around 20%. The intensity is measured as the vertical distance to the flat section. Poverty intensity of controls is slightly over 100 MXN ($\approx$ 4 USD); while the intensity among the treated is slightly below 150 MXN ($\approx$ 7 USD). Taking together these preliminary results give some indication that treated households have lower income inequality

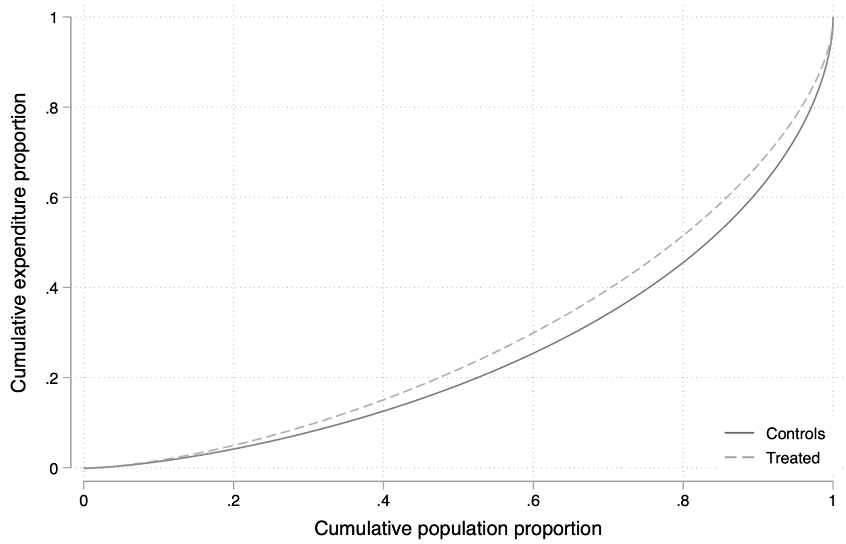

Fig. 4.4 Lorenz Curves for Household Inome for affiliates and non-affiliates

but also lower income. These descriptive findings can be confirmed by estimating the programme's causal effects on income distribution and poverty.

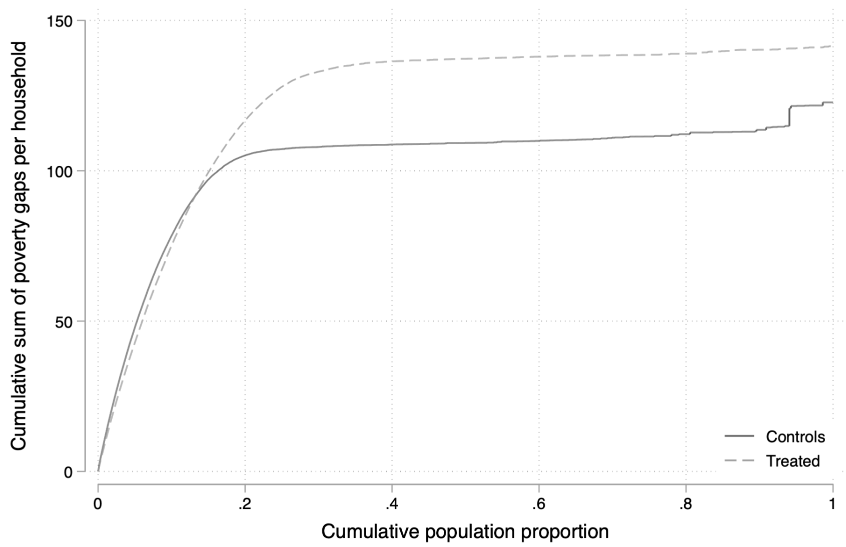

Fig. 4.5 Concentration curves of poverty gaps net of OOP health payments

4.4.2 Econometric analysis

Instrumental variables

Table 4.2 presents results for the instrumental variables models. Column (1) shows ordinary least squares (OLS) regression results of enrolling in SP on the different outcomes; column (2) shows the programme's effects when including municipality and year fixed effects (FE); column (3) shows local average treatment effects (LATE) estimated via instrumental variables (IV); and column (4) estimates treatment effects using a fixed effect instrumental variables approach (IVFE). The instrumental variable is the municipality level roll-out indicator that takes values of one if the municipality participates in SP in a given year and zero otherwise.

Being enrolled in the programme reduced monthly households' OOP healthcare expenditures in 105.7 MXN (≈5 US$) according to OLS estimates, and by 251.1 MXN (≈12 US$) using IV. The difference between the two results indicates the relevance of IV. However, there is no evidence to reject the null hypothesis of regressors' exogeneity as χ^2 pval>.1. Columns (2) and (4) include fixed effects in the analysis to account for persistent differences in municipalities and for time-variant factors common to all households. Fixed effects estimations show a smaller effect of 78 MXN (≈4 US$) and weaker relationship ($p< 0.1$) of SP enrolment on out-of-pocket healthcare spending. The last column shows the results of the IVFE approach. The coefficient shown is the largest among the different approaches, however, it is not statistically significant, moreover, there is no evidence of endogeneity in the treatment variable. The difference between the models when adding fixed effects, provide evidence for the relevance of fixed effects. There are important differences across municipalities and there are factors evolving through the years.

The second panel in Table 4.2 shows the programme's effect on catastrophic payments. According to IV estimates in column (3), being enrolled in the programme reduced the households' probability of having catastrophic expenditures in healthcare by almost 12 percent points in average. The OLS result in column (1) was less than 2 percentage points suggesting endogeneity of enrolling in the programme. Moreover, the test statistic for weak instruments (F-stat=10268), and endogeneity (χ^2 p-val <0.01) confirm the relevance of IV estimation. Nevertheless, when accounting for time and municipality fixed effects, the programme's impact goes to zero.

Panel 3 shows the effect on weighted catastrophic payments that give a greater importance to poorer households as described in equation 4.1. The impact of SP is a reduction of 19.9% and 1.8% for IV and linear model respectively. The greater coefficients on the weighted indicator implies that the policy had more impact on the poorer households. The estimations satisfy F-statistic test for weak instruments and χ^2 test for endogeneity. However, when controlling for fixed effects, the impact of enrolling to the programme is only 1.5 percentage points in the likelihood of having a catastrophic healthcare payment. Given the importance of accounting for fixed effects, and the irrelevance of the instrument in the IVFE approach, column (2) provides the most credible estimates of treatment effects.

Panels 4 and 5 in Table 4.2 show the impact of *Seguro Popular* on the incidence of impoverishing out-of-pocket healthcare payments Δp_i, described as the difference between gross and net of OOP payments poverty headcount; and intensity Δg_i, defined as the difference between gross and net of OOP payments poverty gaps. There is no impact on impoverishment associated to being enrolled in SP. According to IV estimates, SP reduced the likelihood of having impoverishing OOP expenditures by 0.7 percent points. However, the approach does not control for municipality and year fixed effects.

Programme impacts on poverty headcount p^{post} and poverty gap g^{post} overall are examined in panels 6 and 7 of Table 4.2. It is reasonable to think that *Seguro Popular* has an effect on income other than through a reduction in out-of-pocket healthcare payments. According to IV estimations, being enrolled in SP has an effect of reducing the likelihood of being poor by 14.5 percent points and on reducing poverty intensity by 144 MXN (≈7 USD$). However, when including fixed effects, column (3) reports an increase of impoverishment associated with the programme of 2 percent points. These results together with an increase in the pro-poor weighted catastrophic expenditure indicator, may be explained by an inadequate targeting of Seguro Popular on poor population. Evidence suggests that even though healthcare expenses in some categories of expenditure decreased, others, like medications persist. Shortage of medication in the programme, forces users to pay out-of-pocket and this situation would affect poorer population relatively to a greater degree.

Table 4.3, breaks down out-of-pocket healthcare expenditures in 9 different categories of expenditure: outpatient services, prescription drugs, hospitalisation, antenatal care, obstetric services, non-prescription drugs, alternative medicine, orthopaedic services and medical insurance.

Panel 1 in 4.3 refers to the total of out-of-pocket healthcare expenditures as panel 1 in table 4.2. The second panel shows impacts of the programme on outpatient services using the same model specifications as in 4.2. Column (1) shows OLS estimates of 63 Mexican pesos (≈3 USD$). Column (2) shows results of adding fixed effects to control for time-invariant differences in municipalities and for changes occurring each year common to all households. Average treatment effect on the treated is 32 MXN (≈2 US$). IV estimates indicate a local average treatment effect of 23 MXN (≈ 1 US$), moreover, the relevance of this result is confirmed by the test statistic for weak instruments (F-stat=18,347) and endogeneity (χ^2 p-val <0.05). Nevertheless, the IV approach does not include fixed effects that, as described above,

Table 4.2 *Seguro Popular* enrolment impact on households' financial risk

	(1) OLS	(2) FE	(3) IV	(4) IVFE
OOP HE	-105.7***	-78.18*	-251.6***	-649.3
	(38.79)	(39.94)	(75.12)	(521.7)
Observations	122,951	122,947	122,951	123,084
IV-F stat			18,347.5	119.6
χ^2 pval			0.1104	0.235
Catastrophic OOP HE	-0.0192***	0.00197	-0.119***	0.0946
	(0.00300)	(0.00329)	(0.0109)	(0.134)
Observations	123,065	123,061	123,065	123,198
IV-F stat			18,382.5	119.1
χ^2 pval			0.000	0.300
Poor-weighted	-0.0184***	0.0149***	-0.181***	0.209
catastrophic OOP HE	(0.00510)	(0.00553)	(0.0170)	(0.227)
Observations	123,065	123,061	123,065	123,198
IV-F stat			18,382.5	119.1
χ^2 pval			0.000	0.189
OOP HE poverty headcount	-0.00102	0.000166	-0.00732**	-0.0121
	(0.000802)	(0.000882)	(0.00354)	(0.0298)
Observations	123,065	123,061	123,065	123,198
IV-F stat			18,382.5	119.1
χ^2 pval			0.066	0.667
OOP HE poverty gap	-16.59	-17.25	6.689	55.06
	(10.18)	(12.43)	(7.666)	(59.59)
Observations	122,951	122,947	122,951	123,084
IV-F stat			18,347.5	119.6
χ^2 pval			0.1279	0.731
Poverty headcount	-0.00268	0.0197***	-0.145***	0.00268
	(0.00403)	(0.00451)	(0.0108)	(0.147)
Observations	123,065	123,061	123,065	123,198
IV-F stat			18,347.5	119.6
χ^2 pval			0.000	0.914
Poverty gap	-47.90***	-26.58**	-144.6***	-3.070
	(10.39)	(12.74)	(10.59)	(145.0)
Observations	122,951	122,947	122,951	123,084
IV-F stat			18,382.5	119.1
χ^2 pval			0.000	0.856

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

are relevant in this study for addressing bias. Column (4) including fixed effects, finds no effects of the programme on outpatient services. However, the statistics of instrument validity are not satisfied.

The third panel, in contrast to previous studies (Nikoloski and Mossialos, 2018; Wirtz et al., 2012), shows a decrease in prescription drugs expenditures caused by *Seguro Popular* enrolment. However, the effect is below 2 US$. Results of panel 4 indicate no effect of SP on spending on hospitalisation. Panels 5 and 6 of Table 4 4.3 indicate a marginal effect of the programme on antenatal care and obstetrical expenditures. These results are consistent with previous studies suggesting an increased utilisation of antenatal and obstetrical care (Harris and Sosa-Rubi, 2009; Sosa-Rubi et al., 2009). Expenditures on non-prescription drugs and alternative medicine also slightly declined as a result of enrolling in SP. There seems no effect of enrolling in SP on orthopaedic services. Finally, there was a drop in medical insurance spending of less than one US$ associated with SP uptake.

Results of Tables 4.3 and 4.2 together, suggest a decline in out-of-pocket healthcare expenditures. Participants in *Seguro Popular* spent less in most of healthcare categories of expenditure. However, the decline in out-of-pocket spending does not protect the insured against the financial risk of catastrophic healthcare events. Moreover, the programme does not protect against impoverishment caused by out-of-pocket healthcare spending, and it does not seem to be targeting the poor adequately.

Distributional impacts

Figure 4.6 presents the distributional impact of *Seguro Popular* on household income as described in section 4.3.4. The quantile treatment CIC approach estimates the change in income x_i that would have happen in the affiliate group in the absence of SP, using the change in income that did occur in the control group at the quantile that corresponds to that level of income x_i. The programme had positive effects for all quantiles along the income distribution. In terms of policy evaluation, the interest lies on the lower tail, the changes in the available income of the poorer population. The impact between the quantiles 20 and 80 remains practically constant at 100 MXN (5 US$); on the tails the impact is higher: on the left tail of the distribution, the poorest 20%, the effect increases and goes over 200 MXN; while on the right tail of the distribution raises to 300 MXN. The conclusion from Figure 4.6 with respect to income inequality is not clear.

Table 4.4 reports both the Kolmogorov-Smirnov and the Cramer-von-Mises-Smirnov statistics. The first null hypotheses tests whether there is no effect at any quantile τ; and the second, if the effect is constant for all τ. There is statistical evidence of *Seguro Popular* having a positive effect on available income; and that

Table 4.3 *Seguro Popular* enrolment impact on households' OOP health spending

	(1) OLS	(2) FE	(3) IV	(4) IVFE
OOP HE	-105.7***	-78.18*	-251.6***	-649.2
	(38.79)	(39.94)	(75.12)	(521.7)
Observations	122,951	122,947	122,951	122,947
IV-F stat			18,347.5	121.0
χ^2 pval			0.110	0.252
Outpatient services	-63.79***	-32.46***	-23.73***	-174.9
	(15.23)	(5.409)	(5.544)	(124.6)
Observations	122,951	122,951	122,947	122,947
IV-F stat			18,347.5	121.0
χ^2 pval			0.049	0.190
Prescription drugs	-43.33***	-14.02***	-6.552**	-115.4
	(9.988)	(2.516)	(2.696)	(303.6)
Observations	122,951	122,951	122,947	122,947
IV-F stat			18,347.5	121.0
χ^2 pval			0.003	0.157
Hospitalisation	-7.907	-23.59	-24.31	0.896
	(40.95)	(19.36)	(20.53)	(366.2)
Observations	122,951	122,951	122,947	122,947
IV-F stat			18,347.5	121.0
χ^2 pval			0.753	0.936
Antenatal care	-8.091*	-2.246***	-0.477	-59.10
	(4.637)	(0.833)	(0.892)	(41.79)
Observations	122,951	122,951	122,947	122,947
IV-F stat			18,347.5	121.0
χ^2 pval			0.200	0.128
Obstetric services	-42.12**	-14.89***	-12.36***	-242.6
	(16.88)	(2.519)	(3.015)	(148.9)
Observations	122,951	122,951	122,947	122,947
IV-F stat			18,347.5	121.0
χ^2 pval			0.097	0.099
Non-prescription drugs	-8.036**	-5.924***	-3.527*	11.88
	(3.261)	(1.740)	(1.809)	(26.10)
Observations	122,951	122,951	122,947	122,947
IV-F stat			18,347.5	121.0
χ^2 pval			0.621	0.563
Alternative medicine	-21.79**	-2.591***	-0.249	-66.84
Observations	122,951	122,951	122,947	(53.51)
IV-F stat			18,347.5	121.0
χ^2 pval			0.060	0.196
Othopaedic services	-44.84*	5.042	9.716	-191.4
	(25.79)	(13.17)	(13.91)	(144.5)
Observations	122,951	122,951	122,947	122,947
IV-F stat			18,347.5	121.0
χ^2 pval			0.160	0.158
Medical insurance	-14.43	-14.48***	-15.63***	188.8
	(15.90)	(4.336)	(4.623)	(155.9)
Observations	122,951	122,951	122,947	122,947
IV-F stat			18,347.5	121.0
χ^2 pval			0.998	0.172

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

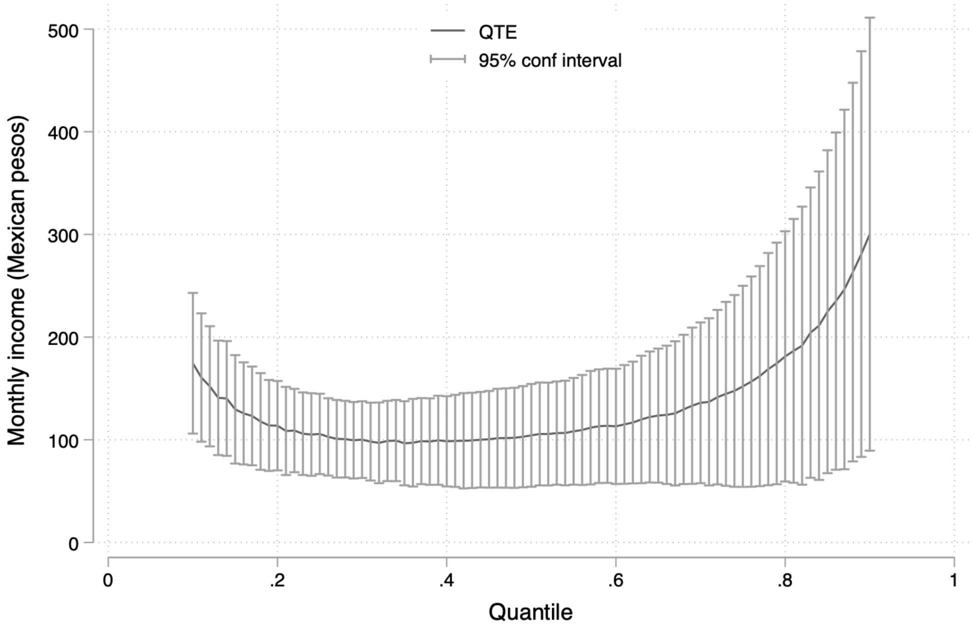

Fig. 4.6 Distributional impact of *Seguro Popular* on income

Table 4.4 Bootstrap inference of treatment distribution effects

Null-hypothesis	KS-statistic	CMS-statistic
No effect: QTE(tau)=0 for all taus	0	0
Constant effect: QTE(tau)=QTE(0.5) for all taus	0.08	0.42
Stochastic dominance: QTE(tau)>0 for all taus	1	1
Stochastic dominance: QTE(tau)<0 for all taus	0	0

the effect is constant and equal to the median for all τ. Moreover, there is evidence of positive stochastic dominance shown in Table 4.4. Figure 4.7 displays the counterfactual and the empirical cumulative distributions. The treatment effects are represented by the horizontal distance between those curves for each quantile. The counterfactual distribution is always slightly to the left of the empirical distribution implying a positive marginal effect on income. Moreover, the effects seem to be constant along the distribution. These results seem to suggest that enrolling in *Seguro Popular* had a positive on income but this effect seems to be constant along the income distribution.

Table 4.5 shows the difference in Gini coefficients of the income distribution derived from the change in OOP healthcare payments induced by *Seguro Popular*. The Gini coefficient of the counterfactual is slightly higher than the one estimated by the model implying that inequality drops due to the introduction of *Seguro Popular*. Although the difference is statistically significant, the drop is only 1 point in the Gini

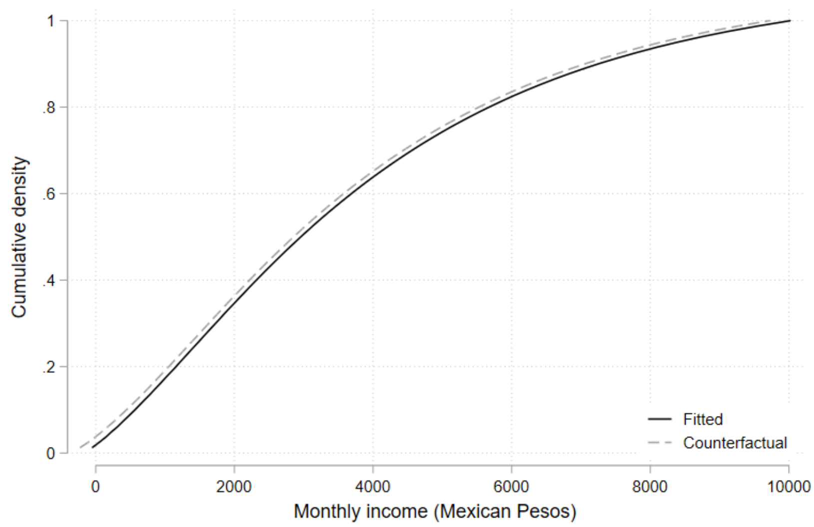

Fig. 4.7 Cumulative distributions of income

Table 4.5 Gini coefficients of CIC transformation distributions

Distribution	No. of obs.	Index value	Std. error	p-value
Counterfactual	81	0.17617176	0.00190081	0.000
Estimated	81	0.1654153	0.00147743	0.000

Test for stat. significant differences with Ho: diff=0 (assuming equal variances)	
F-stat = 19.96272	p-value= 0.0000

Test for stat. significant differences with Ho: diff=0 (large sample assumed)			
Diff. = -.01075646	Std. err. = .00240746	z-stat = -4.47	p-value = 0.0000

coefficient. Figure 4.8 illustrates the change in inequality through comparing the Lorenz curves for affiliates and non-affiliates (and social security non-beneficiaries) and confirms the slight change in inequality as the difference is indiscernible.

Figures 4.9 and 4.10 show quantile treatment effects estimated by the non-parametric instrumental variables model described in 4.3.4. According to the results, there is no evidence of an effect of *Seguro Popular* until the .4 quantile of the distribution. After that, the decline, especially in the top quartile is very sharp and increasing until the top of the distribution. Results indicate that, on average, SP is associated with a 50% drop on out-of-pocket spending at the .75 quantile of the distribution. For the top decile, the programme is associated with a 64% decline, with an average of 495 MXN (≈25 USD) reduction per household.

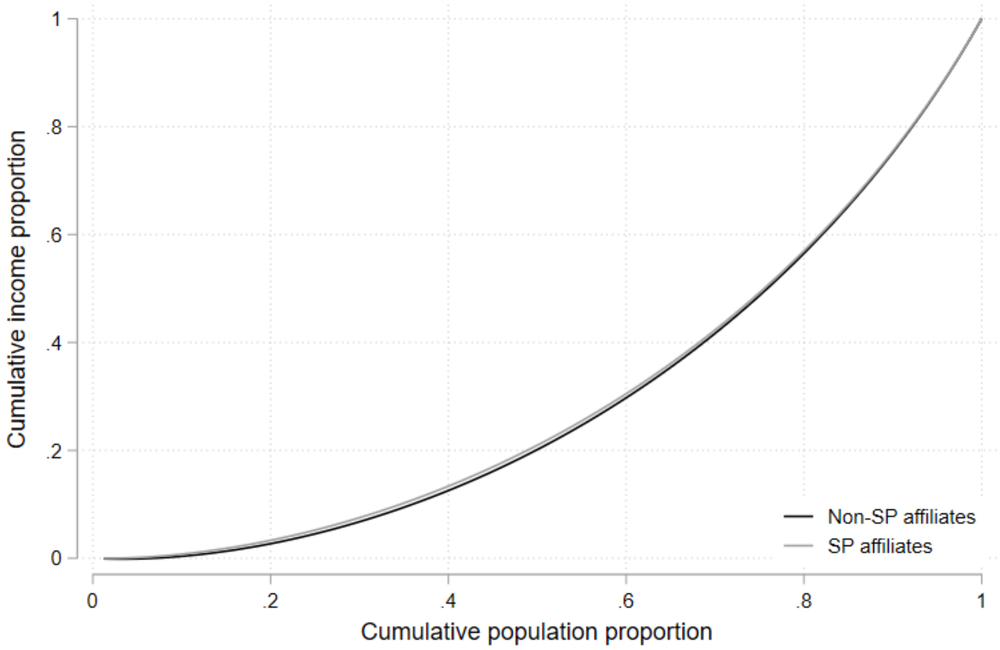

Fig. 4.8 Lorenz curve by affiliation

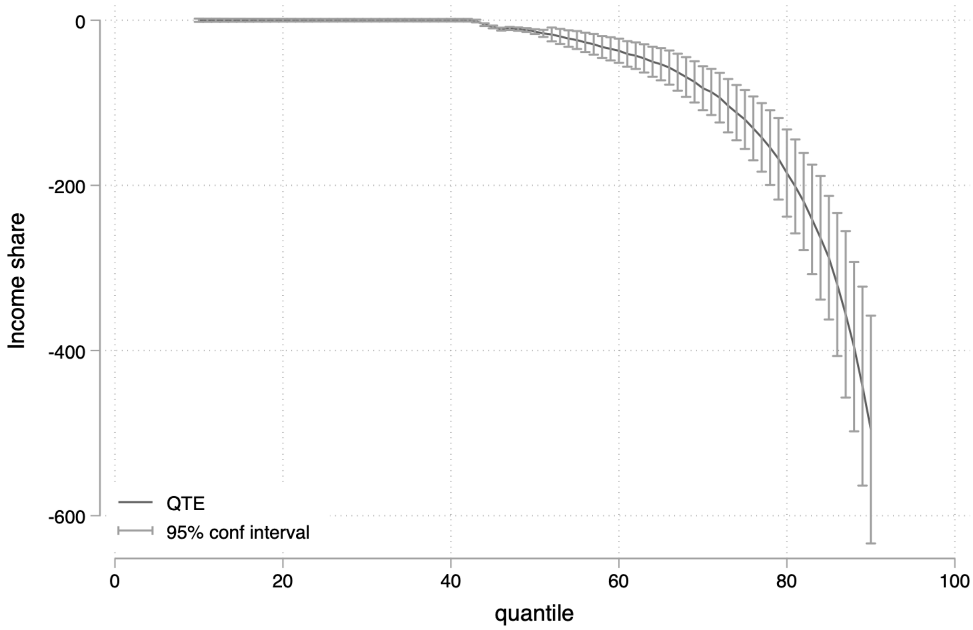

Fig. 4.9 Impact of *Seguro Popular* on OOP health expenditures

Fixed effects

This section uses an alternative definition of the treated population as those living in treated municipalities, therefore the estimated parameter is the intention-to-treat

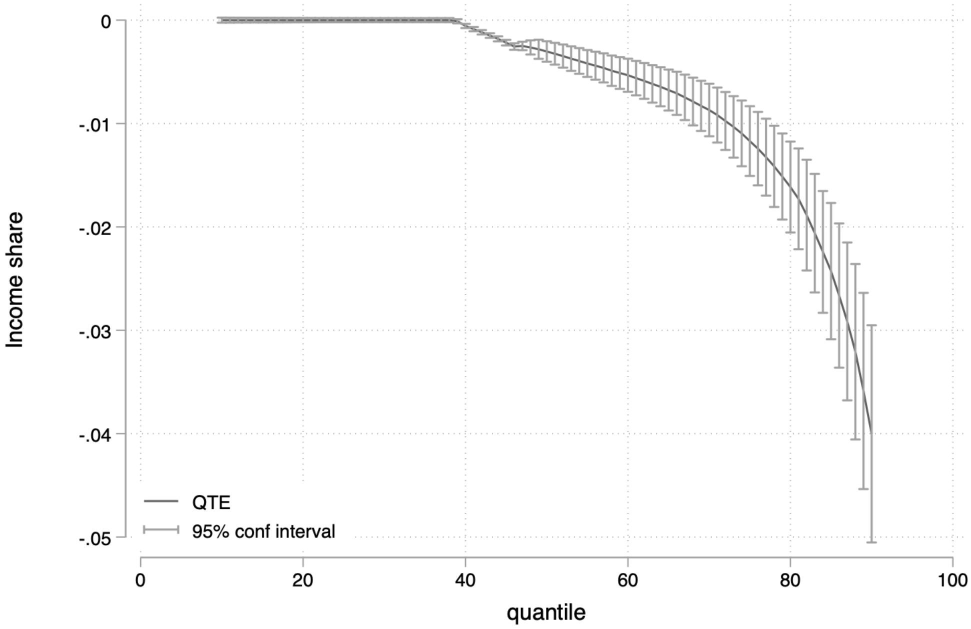

Fig. 4.10 Impact of *Seguro Popular* on OOP health expenditures share of income

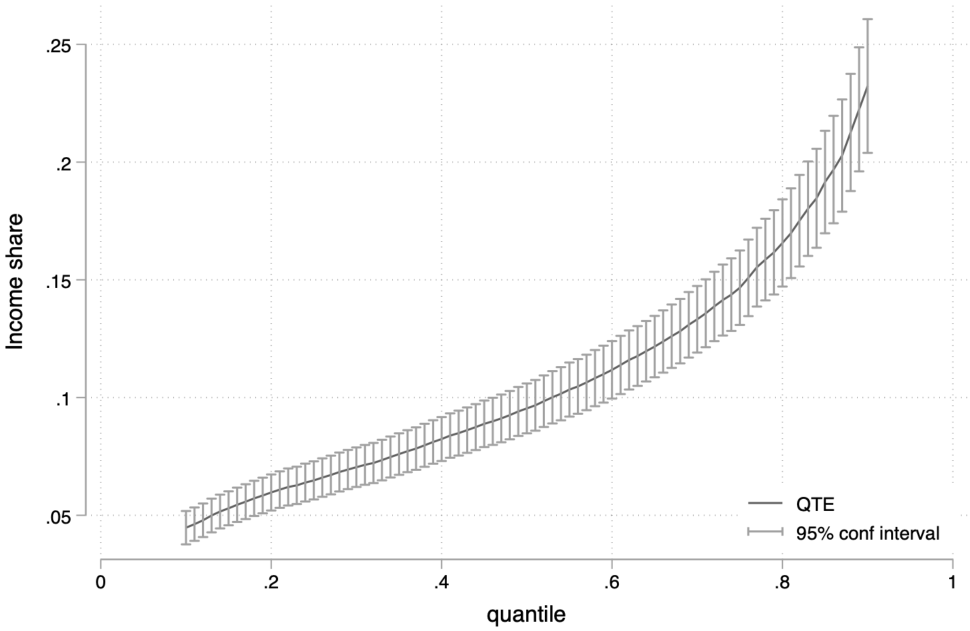

Fig. 4.11 Impact of *Seguro Popular* on households' share of food expenditure of total budget

effect as described in section 4.3.4. Figure 4.12 shows SP's impacts on financial risk variables using the event study approach described by equation 4.5. This parameter estimated is the intention-to-treat effect that can be interpreted as the effect of having the opportunity to enrol. The programme did not have an intent-to-treat effect on any of the outcomes related to financial risk: OOP health expenditures referred to in section 4.3.3 as T_i, catastrophic payments or impoverishment expenditures, both incidence defined as the difference between poverty headcount before and after OOP payments Δp_i, or intensity defined as the before and after OOP payments poverty gaps Δg_i (eq 4.2). There was no impact on after OOP payments poverty measures, both incidence p_i or intensity g_i; or on food expenditures. Any impact on the outcome variables is absorbed by the fixed effects of years or municipalities. Household size and composition have an impact on OOP payments and impoverishment as well as household income level.

Figure 4.13 shows intention-to-treat effects of *Seguro Popular* on out-of-pocket healthcare expenditures broken down into categories of spending. No impact of *Seguro Popular* was found on any of the nine categories, namely outpatient services, prescription drugs, hospitalisation, antenatal care, obstetric services, non-prescription drugs, alternative medicine, orthopaedic services, and health insurance.

These results refer to the effect on having the opportunity to enrol on financial risk. In contrast to the results found in section 4.4.2 that reflect the impact of actually enrolling in *Seguro Popular*, and therefore the difference in impacts found. Results found in this section may be due to a low number of actual affiliates and the possible effect is not reflected on the average of eligible population.

4.5 Discussion

This study assessed the impact of the introduction of *Seguro Popular*, the most ambitious policy expansion towards universality of healthcare coverage in Mexico. Using 10 waves of the National Household Survey on Income and Expenditure (ENIGH), impact of programme affiliation was estimated through different empirical specifications and results showed a decline in out-of-pocket healthcare payments. However, the policy had no effect on catastrophic payments or impoverishing, neither on incidence nor intensity. On average, *Seguro Popular* enrolment was associated with a reduction of 78 MXN (≈4 USD) of household's average OOP health spending and a 50% decline in the top quartile of the payments distribution. Moreover, according to results, the introduction of *Seguro Popular* was responsible for reducing poverty incidence by 2 percentage points and poverty intensity by 26 MXN (≈1 US$). The introduction of *Seguro Popular* had a modest reduction effect on income inequality.

The mixed findings from the current study, specifically reductions on OOP healthcare spending, but no effect on catastrophic spending or impoverishment are

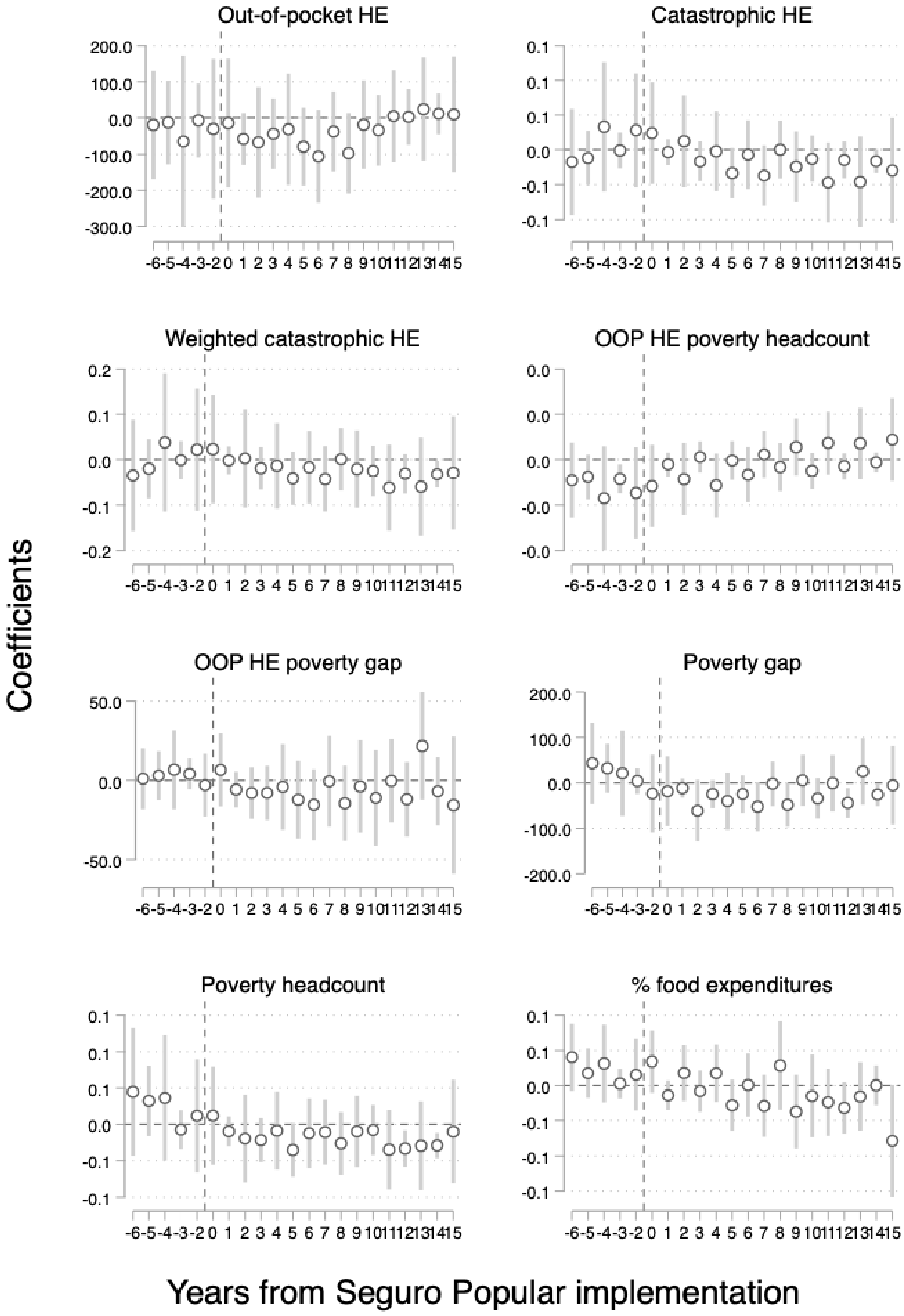

Fig. 4.12 Impact of *Seguro Popular* on households' financial risk

comparable to those found in previous studies. There is extensively documented literature on the impact of SP on OOP healthcare spending and catastrophic payments. Most studies find some degree of financial protection impact, although in some cases results found are limited. King et al. (2009) estimates *Seguro Popular*

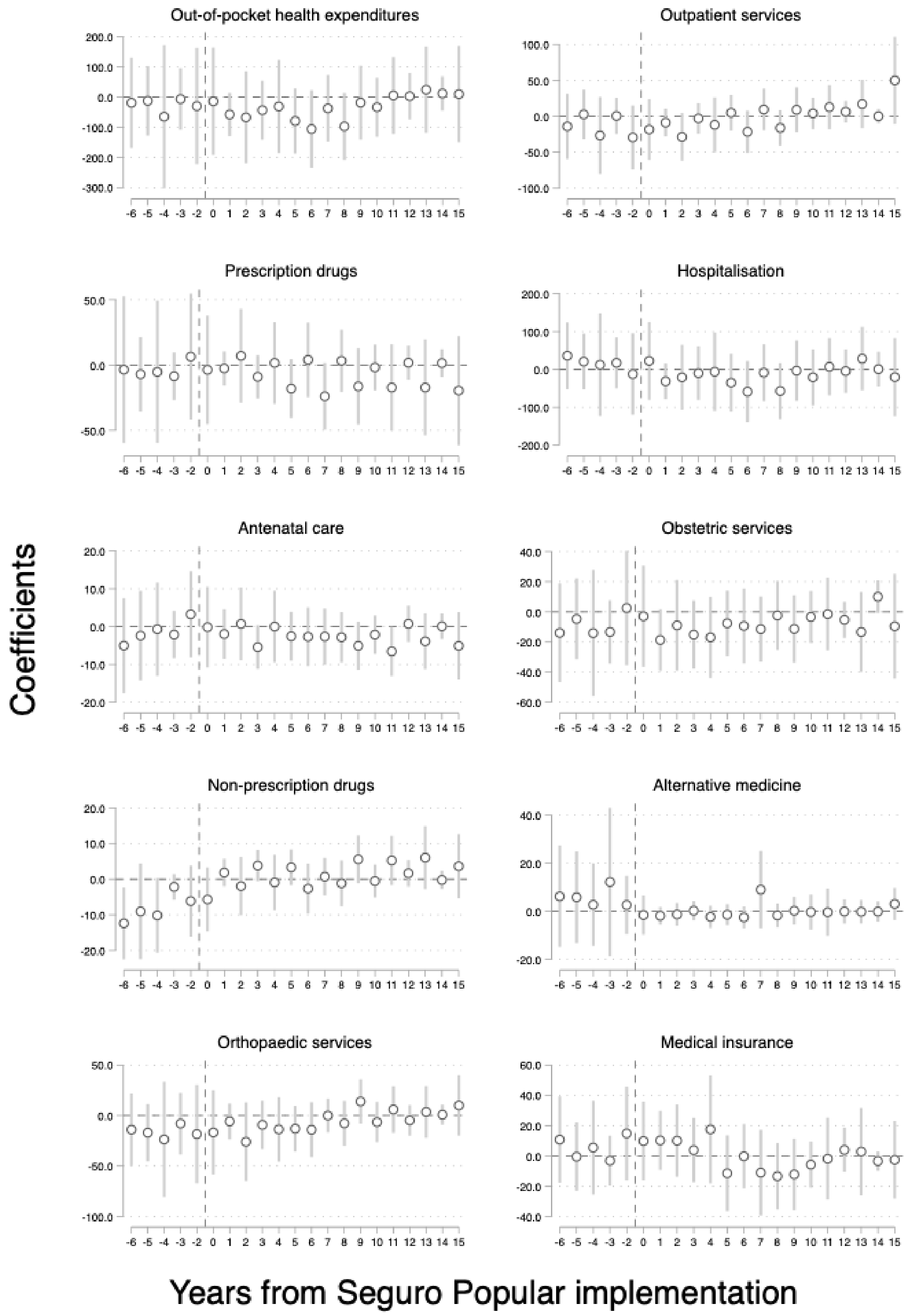

Fig. 4.13 Impact of SP on households' OOP HE for different spending categories

intention-to-treat effects of 23% reduction in catastrophic expenditures, and 30% on poorer population. For OOP healthcare spending, overall the impact found was a reduction of 31% but only on poorer population. Moreover, they found no effects on medication spending or health devices. Sosa-Rubí et al. (2011), using data from the *Seguro Popular* Survey evaluation, found protective effect of *Seguro Popular* on out-of-pocket payments in outpatient services and hospitalisation in rural and urban areas; and an impact on catastrophic payments only in urban zones. Nikoloski and Mossialos (2018), using the *Encuesta Nacional de Salud y Nutrición* (National Survey on Health and Nutrition ENSANUT) 2006 and 2012, analyse the effect of *Seguro Popular* and social security on out of pocket and catastrophic healthcare expenditures; and found no link on SP membership and OOP health spending, but a robust although small effect of SP on reducing catastrophic health expenditures.

On healthcare categories of spending, Wirtz et al. (2012), using the same data as this study, but examining a shorter period (2004 to 2008), analyse the effect of health insurance on households' probability and amount of OOP spending for medicines. They found that SP reduced the probability of out-of-pocket health spending by 9.7% ($P < 0.05$) and the annual amount spent by US$24.51 but showed no significant effect on out-of-pocket spending on medicines in comparison to households without health insurance. In contrast, the present study finds an impact of SP on healthcare OOP payments in prescribed and non-prescribed medications, but also for outpatient services, obstetric, antenatal services and alternative medicine (see table 4.3). The difference in findings may be due to the difference in the study time frame, while this study considers 8 waves over 16 years, almost the whole duration of the programme, previous studies look at shorter periods. It is possible that having additional data increasingly available, allows for analyses with a higher level of precision. As the programme was replaced recently, it is expected that more analyses covering the whole period will be available soon.

Compared to the extensive literature on *Seguro Popular*'s impact on OOP healthcare spending, evidence on impoverishment is limited. Knaul et al. (2006), in an early study on SP, found an association between the expansion of *Seguro Popular* and the decline of catastrophic and impoverishing spending. However, they had insufficient data to determine a causal relationship. Nikoloski and Mossialos (2018) found some effect of *Seguro Popular* sheltering households from impoverishing; however, the magnitude was small. Knaul et al. (2018), in contrast, find a statistically significant impact of *Seguro Popular* on reducing the likelihood that households will incur impoverishing expenditures by 42%. García-Díaz and Sosa-Rubí (2011) find that *Seguro Popular* affiliates have the lowest distributional poverty impact compared to other poverty reducing policies. The present analysis found that *Seguro Popular* had an impact of reducing the incidence of impoverishment by 0.8 percent points; this is an important reduction as incidence was estimated in 1.1% in average. This finding has broad implications on how to design health insurance policy to reduce impoverishment. Knaul et al. (2018) using the same dataset as this study for years 2004-2012 applied propensity score matching methods on affiliated population and found statistically significant effects on reducing impoverishing out-of-pocket healthcare expenditures but not on catastrophic expenditures. Those results are consistent with the current study.

There are some limitations to this study, mostly related to the availability of data. While the data used for this study is a repeated cross section, panel methods would be preferable to pooled regression. A panel would account for part of the bias caused by the endogeneity on the individual decision to enrol. A repeated cross-section provides a time dimension, and the individual dimension. However, differences across households over time are not observed. Being able to control for such differences would improve estimations, specially long-run SP effects. Moreover, there is no data on the exact time households enrolled in *Seguro Popular*. Knowing that variable would allow us to explore differences in exposure to treatment. Another limitation is related to the programme's design. The nature of the voluntary insurance introduces bias as described before. Moreover, the roll-out at the state and municipality level was not random, but rather arbitrary (affected by political considerations and constrained by logistics). Finally, even though self-selection issues were addressed, there may be households, especially in the lower income deciles, that suffered health shocks but were unable to pay out-of-pocket for a service and, therefore, forgo or postpone health care. In those cases, self-selection persisted.

This study contributes to the literature on public health insurance in different aspects. This is one of the few analyses on the long run effects of *Seguro Popular*, covering almost its entire duration, from the pilot phase in 2002, to 2018, one year before its cancellation. A contribution to the methodological approach has been to combine instrumental variables with fixed effects approaches. Previous analyses have relied on those methods separately on multiple occasions but there is no study using an IVFE approach. Another contribution is the analysis of heterogeneity in the outcomes using non linear models. Existing analyses of *Seguro Popular* are almost entirely on the average effect using linear causal inference approaches. In this study, quantile treatment effects of the policy are estimated by two approaches changes in changes (Athey and Imbens, 2006) and an instrumental variable quantile approach proposed by Frölich and Melly (2013). Quantile treatment effects were seldom used to analyse *Seguro Popular*, only Huffman and van Gameren (2019) who focused on the supply-side factors and Barofsky (2011) on the early years of the programme.

Finally, this work contributes to examining alternative paths through which *Seguro Popular* may have an impact on welfare. Most poverty analysis of *Seguro*

Popular focus on the OOP impoverishing impact. However, the causal path is not necessarily through reduction in OOP healthcare payments. There are different channels in which *Seguro Popular* could impact households' income. The coverage expansion has the direct effect of transferring income through free healthcare, but the indirect effects on health could have impacts on households' income and welfare. Two of the channels through which improved health can have a positive impact on income are: improved health increases productivity of individuals; and, reducing the time individuals spend taking care of household members after health shocks. Del Valle (2014) finds that SP increases labour supply by allowing caregivers to continue working by reducing the burden of caretaking dependants when facing illness. This increase in productivity will positively impact labour and welfare. This study explores the impact of *Seguro Popular* on poverty incidence and intensity through time and finds that the policy expansion reduces both. Moreover, these results suggest an additional impact of on households' ability to smooth consumption through time, a poverty reduction effect and an increase in welfare.

Chapter 5

Conclusions

The purpose of this thesis is to provide an evaluation of *Seguro Popular*, an ambitious policy reform introduced by the Mexican government to address inequity in access to healthcare services. Its goal was to grant healthcare coverage to the population without social security protection from the lowest deciles of income. This work examines the effectiveness of *Seguro Popular* in achieving this aim over three main chapters: the first is an assessment of SP impacts on health outcomes and healthcare utilisation; the second examines its distributional impacts and health inequality; and the third analyses the financial protection effect of SP against out-of-pocket healthcare payments, catastrophic spending and impoverishment.

Seguro Popular operated from 2004 to the end of 2019, including an initial two year pilot phase. Over that period, there were important improvements in health outcomes, healthcare utilisation and financial protection in Mexico. Evidence suggests that some of those advances are directly associated to the healthcare coverage expansion of *Seguro Popular*. Before the implementation of *Seguro Popular*, in 2000, 58.6% of people did not have any healthcare coverage; by 2018, that proportion was reduced to 16.2%[1]. *Seguro Popular* was implemented progressively at the geographical and the individual level. Moreover, the package of interventions covered increased over time. It is possible that the sequence in the introduction of the programme on individual and geographical enrolment and on the interventions covered is the cause for the differential timing in impacts.

Evaluations can be misleading if timing is not taken into account. For example, if an evaluation is conducted before the treated group has been exposed to treatment long enough to benefit from it, the impact would be underestimated. Moreover, if there are differences in timing of exposure to treatment across groups, estimates of

[1]Coneval. Evolución de las dimensiones de la pobreza 1990-2018. https://www.coneval.org.mx/Medicion/Paginas/Evolucion-de-las-dimensiones-de-pobreza-.aspx

impacts may vary. King and Behrman (2009) identify three sources of variation in impact evaluations estimates; the first is leads and lags in the implementation caused by organisational factors; the second are spillover effects, the programme effects may intensify over time affecting estimates of programme impact; the third source is heterogeneity in impact on populations from different cohorts, or with differences in other characteristics. In the case of *Seguro Popular*, timing is crucial to estimate treatment effects.

To account for organisational lags in the implementation and spillover effects, in Chapters 2 and 4, programme impacts were estimated using the progressive timing in the implementation as an identification strategy. Municipalities were rolled-out in phases but also people within municipalities were affiliating gradually. Intensity, measured as the level of penetration at the population level, varies in time: in general, the longer a municipality is exposed, the more families become beneficiaries. Finally, the number of interventions covered by the programme rose over time. SP started with a basic set of 90 essential health interventions and 142 medications associated with them. By 2018, the number of interventions had risen to 294 with 664 medications (Chemor Ruiz et al., 2018). Chapter 3 also accounts for operational lags and also for heterogeneity in impact on population from different cohorts by measuring impacts on children who were exposed to *Seguro Popular* during a critical period that occurs before five years of age.

Taking all these elements into account, one would expect that the impacts might not be immediate, but instead it would take time for the full impact of the reform to manifest. Moreover, there may be different for people from different cohorts or with different characteristics. Ignoring these sources of variation may introduce a downward bias on treatment effect estimations. Figure 5.1, adapted from King and Behrman (2009), shows how programme impact can change over time. In the case of *Seguro Popular*, programme impacts on health outcomes may behave like the upper panel in Fig 5.1, suggesting that an early evaluation might provide a misleading conclusion. Therefore, later evaluations would provide more reliable estimates of *Seguro Popular* true impact.

Evidence confirms that *Seguro Popular*'s impact on health outcomes varied with the timing in introduction. Early studies found that the programme had from non-existent in most cases to very mild treatment effects. Barros et al. (2008) and King et al. (2009) did not find any effects of SP on health outcomes among the eligible population. More recent studies find positive impacts of SP on health outcomes. Pfutze (2014) finds a reduction in infant mortality rate by 5 in 1000 births; while Pfutze (2015) finds evidence of reduced risk of a miscarriage of 0.04% for each percentage point in crease in coverage. Conti and Ginja (2020) estimate intent-to-treat effects and find that SP reduced infant mortality by 10% in poor municipalities.

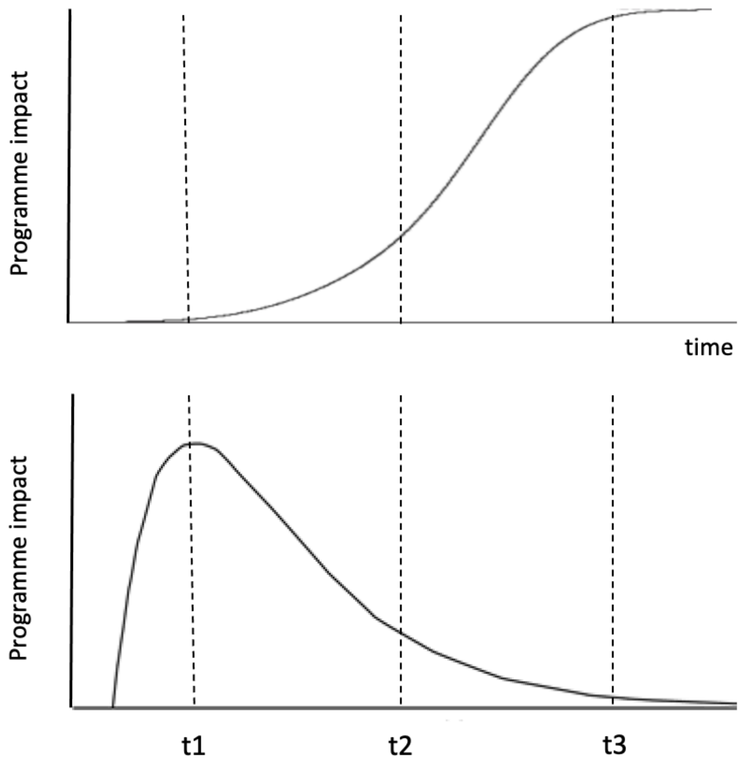

Fig. 5.1 Timing of Impacts and Evaluations

The results in this study are consistent with the existent evidence. The estimated programme impact on health outcomes found in Chapter 2 are comparable with the early evaluations literature with data from the early years of the programme; while the effect on mortality rates found using data for the whole period, is similar to the evidence from later studies. There is a positive impact of *Seguro Popular* on infant and adult mortality after the programme has been operating for at least 10 years in the municipality.

In contrast to the timing in the impact on health outcomes, evidence suggests that *Seguro Popular* had immediate impacts on reducing out-of-pocket healthcare payments, catastrophic and impoverishing expenditures. *Seguro Popular* as a public health insurance, its priority was to protect the population against healthcare shocks with catastrophic consequences. For that reason, the package of interventions included in the programme by the time it was implemented, covered 90% of disease burden (Ruvalcaba and Vargas, 2010). Early descriptive studies find associations between *Seguro Popular* membership and decline in catastrophic healthcare payments (Gakidou et al., 2006; Knaul et al., 2006; Scott, 2006) and a reduction in the likelihood to incur impoverishment (Knaul et al., 2006). Later on, several studies found causal relationships of SP and reduction of out-of-pocket healthcare payments (Barros et al., 2008; Sosa-Rubí et al., 2011), substantial decline in catastrophic payments

(Galárraga et al., 2010; King et al., 2009; Sosa-Rubí et al., 2011) and protection against impoverishment García-Díaz and Sosa-Rubí (2011). However, programme impacts found in more recent studies seem to be waning, suggesting that impacts are similar to those illustrated in the lower panel of Figure 5.1. A more recent study found no relationship between affiliation to *Seguro Popular* and out-of-pocket spending for medicines (Wirtz et al., 2012); other studies report limited impacts on catastrophic healthcare spending (Grogger et al., 2015; Nikoloski and Mossialos, 2018) or no effect at all (Knaul et al., 2018); another study found limited effects on impoverishment (García-Díaz et al., 2018). Consistent with the timing of impacts found in those studies, the results of Chapter 4 of this thesis suggest an impact of *Seguro Popular* in reducing out-of-pocket healthcare payments, but no change catastrophic healthcare payments and impoverishment, implying immediate and fading effects.

One of the advantages of this study compared to previous literature could be the availability of more recent data. While this study looks at the programme in its entire duration, past studies look at shorter periods, missing the last years of the programme. *Seguro Popular* has been implementing improvements and amendments to the original design over the years as problems arise. From 2014, a number changes and adjustments were implemented on regularisation of transferences, management of funds, financial monitoring, among others (Chemor Ruiz et al., 2018). From 2012 to 2018, 142 medicines and 9 interventions were included (Chemor Ruiz et al., 2018). Despite the continuous expansion of *Seguro Popular* in the last years, the impacts on out-of-pocket expenses have shown decreasing returns.

The use of longitudinal data on individuals in Chapters 2 and 3 has an advantage over most evaluations of *Seguro Popular* on health outcomes using cross-sectional data as self-selection bias is not completely corrected. Chapter 4 analysing repeated cross-sectional data addresses the endogenity controlling for unobservable characteristics of individuals through instrumental variables. The model specifications used in Chapter 2 take into account heterogeneity in time of exposure and in programme intensity on time and space and heterogeneity according to income level. Examining several years of data in Chapters 2 and 4 captures the long-term policy impacts missed by earlier studies. The comparison of two different data structures and two different methods in Chapter 3 provides insight on different biases on the conclusions due to sensitivity of results. Examining distributional as opposed to average effects provides valuable information for policy design on the importance of programme targeting.

Seguro Popular left major healthcare challenges unresolved. Universal coverage was not attained. Between 2008 and 2018, the proportion of population with no access to healthcare fell from 38.4% to 16.2%[2]. However, in the last years enrolment in *Seguro Popular* has been declining, falling to 45% of affilition after reaching almost 60%, perhaps related to the low quality perception. Despite substantial expansion, there is still a highly vulnerable proportion of the population with no healthcare coverage (Reyes-Morales et al., 2020).

Another limitation of SP was the catalog of services provided (CAUSES). Even though there were important additions over the years, the inventory of primary-care and ambulatory services were not adequate to serve the demand for preventive services (Chemor Ruiz et al., 2018). This situation was especially serious for the poorest sector of the population. Colchero et al. (2019) found that in small localities only 32% of people reporting a health problem received care in a public institution. Moreover, belonging to a higher income group was associated to higher likelihood of receiving healthcare.

Out-of-pocket healthcare payments are still problematic. According to Shamah-Levy et al. (2020), 60% of people reported paying out-of-pocket for medications the last time they needed them. Overall, 40% of health spending is out-of-pocket. Medication shortages are another persistent challenge for general population but are more serious for poorer population. In 2014, the share of out-of-pocket healthcare spending of total income in the poorer quintile was 16%, while that share for the richest quintile was only 4% (Bautista-Arredondo et al., 2014).

Challenges related to the transfer and allocation of funds remain pending. Nigenda et al. (2015) identified delays in the transfer of funds from the federal to the state level up to six months; lack of adherence to expenditure targets of states and unauthorised use of financial resources for contracting personnel without adequate training, paying excessive prices for drugs and other goods, among other inappropriate use of resources. The unmet goals of *Seguro Popular* together with documented cases of corruption and, importantly, ideological and political reasons provided the rationale to replace the programme with another system (Reich, 2020).

On January 1st 2020, *Instituto de Salud para el Bienestar* (INSABI), a system of universal and free access to health services and associated medicines, was introduced to provide healthcare coverage to 57%[3] of citizens (71.7 million[4] people) with no social security protection. INSABI is a centralised health system with integrated public

[2]Coneval. Evolución de las dimensiones de la pobreza 1990-2018. https://www.coneval.org.mx/Medicion/Paginas/Evolucion-de-las-dimensiones-de-pobreza-.aspx

[3]Coneval. Medición de la pobreza. Evolución de las dimensiones de la pobreza 1990-2018. https://www.coneval.org.mx/Medicion/Paginas/Evolucion-de-las-dimensiones-de-pobreza-.aspx

[4]These 71 million people include the 53.5 million previously covered by *Seguro Popular*.[5]

[5]*Seguro Popular*. Personas afiliadas: http://www.transparencia.seguro-popular.gob.mx/index.php/transparencia-focalizada/21-pesonas-afiliadas.

financing and delivery, where the single funder and provider is the federal government. In contrast, *Seguro Popular* had a different structure. It did not provide healthcare, but instead was a financial mechanism that transferred resources to state-based agencies (REPSS[6]) that in turn managed the resources and local authorities provided healthcare. The set of services covered were defined by a federal agency (CNPSS[7]) and included a catalog of services with 294 interventions (CAUSES[8]) provided by primary and secondary level health facilities; and 66 catastrophic conditions delivered in tertiary institutions with resources from a separate fund (FPGC[9]). In addition, it provided all health services to children up to 5 years old (SM SXXI[10]) (Reich, 2020).

The new system starts operating with uncertainties about the services included, its operation rules and financial sources. SP covered 294 interventions but some others were not financially feasible to be included in its scope. Some examples are myocardial infarction for individuals older than 65 years and management of renal failure with dialysis (Chemor Ruiz et al., 2018). Including some of those interventions could collapse the whole system. However, the reform is not specific about an intervention package; but only explicitly states that the coverage includes at least primary level outpatient services, as well as outpatient consultation and hospitalisation for basic medical specialties, internal surgery, general surgery, obstetrics gynaecology, paediatrics and geriatrics, at the secondary level, and associated medicines[11]. However, the cost to deliver the promised services is five times the budget assigned to INSABI in 2020.

An important lesson to draw from *Seguro Popular* was its constant monitoring and evaluation. *Seguro Popular* had well defined financial and operation rules; it was tested in a two-year pilot phase in 5 states; and was constantly monitored and evaluated. An experimental evaluation previously described was conducted at the time of introduction of *Seguro Popular*, with promising findings. Currently, INSABI has no clear plans for evaluating impacts of the new policies (Reich, 2020; Reyes-Morales et al., 2020). There are many challenges ahead for the new system and the entire Mexican health system especially in the context of an epidemiological transition, with and increasing prevalence of resource-intensive conditions as cardiovascular diseases, diabetes and cancer. It is crucial to assess new policies in contrast to the

[6]Régimen Estatal de Protección Social en Salud
[7]Comisión Nacional de Protección Social en Salud
[8]Catálogo Universal de Servicios de Salud
[9]Fondo de Protección contra Gastos Catastróficos
[10]Seguro Médico Siglo XXI
[11]Act Project for reforming the Ley General de Salud de Mexico. https://infosen.senado.gob.mx/sgsp/gaceta/64/1/2019-07-03-1/assets/documentos/Ini_Delgado.pdf

ones that precede them. Impact evaluations should be the basis for informing the decision to introduce, reform or cancel public policies.

Finally, whether INSABI can face the major challenges in healthcare for the uninsured is uncertain, it may not address the most important cause of inequality in healthcare access in Mexico, namely, the fragmented health system between social security and social assistance. Social security affiliates receive higher quality and more effective healthcare than non-affiliates due to the fact of being employed in the informal sector. One step towards a more equal system would be to match the per capita federal spending on health between the uninsured and social security affiliates. To achieve that, it would be necessary to increase health spending by 1.5 GDP points (Antón et al., 2012). However, during *Seguro Popular* years health spending increased only 1 GDP point. As long as the health system continues to be linked to labour status and uninsured population do not have access to the full package of social security benefits, the deeply engrained problem of inequality will not be addressed. Without a restructure of the health and financial systems the challenge seems unrealisable.

Future research should focus on the previously describedchallenging main areas to inform policy on how to improve the health system. Immediate assessments of INSABI specifically on access, out-of-pocket expenditures and health outcomes are necessary to analyse its short term performance. Moreover, further research on the persistent problems in the Mexican health system like catastrophic payments, out-of-pocket spending on medications, and the reasons behind the low healthcare affiliation and its recent decline should be prioritised. Another line of research should focus on the quality of healthcare; the perceived low quality of *Seguro Popular* compared to social security. There is not enough evidence on the quality aspects of the recently disappeared scheme. A comparison to INSABI may inform decision makers. Lastly, it is important to dig deeper to understand the problem of inequity the health system. This research agenda will provide with scientific evidence that may be useful to set the basis for a truly universal healthcare system in Mexico.

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